AMERICAN SOCIETY OF CIVIL ENGINEERS

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CXVI.

THE NEW PORTAGE BRIDGE.

A Paper by GEORGE S. MORISON, C. E., Member of the Society.

PRESENTED NOVEMBER 27TH, 1875.

On Thursday, May 6th, 1875, before daybreak, the famous timber viaduct over the Genesee river at Portage, on the Buffalo division of the Erie Railway, took fire, and was totally destroyed. The viaduct was 850 feet long and 234 feet high, built in spans of 50 feet each, each timber pier being formed of three bents placed side by side, and resting on stone piers. The destruction of the viaduct was complete, not a single stick of timber being left unburnt; the masonry in the bottom of the chasm through which the river flows, was badly scarred by the fire; that on the banks was nearly destroyed. The magnitude of the structure, and the fact that the railway had another line over which the Buffalo business could for a time be handled, made the erection of any temporary structure inexpedient, and it was at once decided to rebuild in iron.

On Monday, May 10th, the contract for the iron work was let to the Watson Manufacturing Co. of Paterson, the bridge to be built according to plans prepared by and under the direction of the writer. Arrangements were made during the same week for repairing the masonry; a new abutment was built at each end, the piers on the east bank were taken down to below the surface of the ground and new ones built; those on the west bank were abandoned, and new ones built 18 feet from them, the support of the old structure being considered too near the precipice on this side of the chasm; three of the piers in the chasm were abandoned, and the others were repaired and provided with large pedestal stones to sustain the iron work. In repairing the masonry

extensive use was made of beton coignet, with which the upper surfaces of the piers were covered, and in which the portions of the piers exposed to the constant action of frost and water were encased.* The iron work was considerably delayed by failures of the rolling mills to make prompt delivery, and the first iron column was not raised till June 13th. On July 29th the iron was all in position, on the following day the track was laid across, and on Saturday, July 31st, the bridge was formally tested in the presence of the principal officers of the railway, besides a large assemblage of spectators, and immediately thrown open for traffic.

The new structure is of the same general character as other iron viaducts recently erected by American engineers, differing from them in size and in detail rather than in any principle of construction.

The Buffalo division of the Erie Railway for 20 miles east and 30 miles west of Portage, is a single track line, but the laying of a second track has been contemplated for a number of years. The bridge stands at the foot of a grade of 1 in 100 about 1½ miles long, which it has been proposed to reduce by raising the track on the bridge. As the straitened financial circumstances of the company demanded the least possible immediate outlay, it was determined to build a single track structure, but one which could be altered at no great expense to accommodate a second track, and on which the track might be raised about 20 feet, were such a change thought expedient. In designing the new bridge, the supporting columns were all made strong enough to carry a double track superstructure; the trusses are of proper strength for a single track only, but placed 20 feet between centres, and made of a narrow pattern, so that when a second track is needed, the strength can be doubled by placing similar trusses immediately alongside of them. Should it also be thought expedient to change the grade, the capitals have a sufficient width on each side of the truss-bearings to support the foot-shoes of a new truss, the bottom chord of which would be directly above the floor of the present bridge, which new truss could be erected without disturbing the running of trains, and on its completion, the new double-track floor could be laid on the upper chord of the new truss. It is believed, however, that the reduction of grade can be made more easily by changing the location west of the bridge.

The iron viaduct has ten spans of 50 feet each, two of 100 feet, and one of 118 feet, a 50 feet span being placed between each of the long spans. The trusses are supported by wrought iron columns, the ends of two

* Although this work has has not yet had the trial of time, it is believed that no better or more economical method can be found of restoring defective masonry to its original strength.

adjacent trusses resting upon a single column. The pair of columns supporting the opposite trusses are in the same vertical plane, but are inclined towards each other with a batter of 1 in 8; they are united with wrought iron struts 25 feet apart and diagonal tie rods, thus forming a two-post bent; each column is connected with the parallel column of the adjoining bent by a similar arrangement of struts and diagonal ties; four columns with the connecting bracing are thus made to form a single skeleton tower, 20 feet wide and 50 feet long on the top, surmounted by a 50 feet span of bridge, having the same length at the bottom and a width varying with the height of the tower. There are six of these towers, called for convenience of reference, A, B, C, D, E and F, of which towers D and E are the largest, they having a total height, from masonry to rail of 203 feet 8 inches, and being 69 feet 8 inches wide between centres of columns at the base. (See plates.)

The trusses of the superstructure are built of the standard of strength in general use on the Erie Railway; they are proportioned to carry a moving load of 3 000 pounds per running foot, with an excessive load of 5 000 pounds per foot, the latter being used in proportioning the floor system between panel points and the variable element in the web system, with a maximum tensile strain of 10 000 pounds per square inch. The towers are built to carry a moving load of 5 400 pounds per running foot, in addition to the estimated weight of a double track superstructure; they are also calculated to resist a wind pressure, at right angles to the bridge, of 30 pounds per square foot, exerted on the entire surface of the structure and of a train of cars on top, and one of 50 pounds per square foot exerted on the surface of the structure alone.*

The maximum compressive strain per square inch allowed in the columns, is 6 600 pounds, and the maximum texile strain allowed in the diagonals, 15 000 pounds; as however the diagonals have an important stiffening function to perform, independant of the resistence to wind effects, it was thought best to use no rods of a less diameter than 1¼ inches, which size is used everywhere, except in the upper section of the towers which sustain the long spans, the batter of the posts making the strains on all the diagonals comparatively uniform.

* These strains have been estimated on three suppositions. *First.*—When the bridge is covered with a maximum load of 5 400 pounds per foot, besides the weight of a double track superstructure, with no wind pressure. *Second.*—When it is covered with two lines of loaded box cars, weighing together 2 800 pounds per foot, and the wind exerts a pressure of 30 pounds per foot, at right angles to the axis of the bridge. *Third.*—When the wind blows with a pressure of 50 pounds per square foot on the bridge alone with a single track superstructure. This last supposition is the one approaching nearest to the condition which would overturn the bridge, but it shows nowhere a negative result in the pressure on the columns.

The columns rest upon cast iron pedestals ; those on the north side of the bridge being secured by dowels to a cast iron plate sunk in the masonry, and those on the south side being placed on rollers, rolling at right angles to the axis of the bridge ; the pedestals are connected by eye-bars to take up the thrust due to the inclination of the posts, and are kept apart by struts adjustable with wedges, to resist the inward thrust caused by screwing up the diagonals. This arrangement, which is not needed in smaller iron structures, was thought important here, in order to relieve the masonry, which is old, from all possible thrust, while the use of an adjustable strut makes it possible to throw all the tensile strain due to the inclination of the posts, on the horizontal ties, leaving the diagonals to perform only their function of wind and vibration stiffness.

The columns are made in 25 feet lengths. They are formed of three plates and four angle irons, with a lacing on the fourth side, so that the interior of the column is accessible for painting, The angles are all $4 \times 4 \times \frac{1}{2}$ inches, and the plates are all 15 inches wide ; the back plate is of the same thickness for the whole length of each column, while the thickness of the side plates is varied to provide for the increased strains in the lower sections. The thinnest plate used is $\frac{1}{2}$ inch thick, this being the back plate of the columns on which the ends of two short spans are carried.

The ends of the several lengths are squared and faced, and they rest directly upon one another without joint boxes of any kind ; the upper end of each length is fitted with two projecting plates which form a tenon; the length above fits over the tenon plates and is secured to the lower length by a turned pin of $1\frac{7}{8}$ inch diameter passing through carefully bored holes ; this same pin serves for the attachment of the longitudinal rods. A second pin at right angles to this one, is placed a few inches below the joint and forms the attachment for the transverse strut and ties ; the end of the strut fits in between the side plates of the column and is held by the pin ; the diagonal-ties are attached to the pin on each side of the column, they being everywhere in pairs. The longitudinal strut, which is nearly 50 feet long, is built in the form of a light lattice truss, is 2 feet deep and 1 foot wide, with the ends squared and butting against the side of the column; it is further secured by bolting it to lugs attached to the side plates and is stiffened by angle iron braces connecting it with the corresponding transverse struts, 10 feet from each end. The lengths of the transverse struts vary from 20 feet at the top of the tower to 64 feet at lowest joint in the main towers; the three lower struts are made in two parts, connected with splice plates and supported by a light central post; the first and second struts are further stiffened by an intermediate longi-

tudinal strut and a system of horizontal diagonal rods. The wrought iron columns are surmounted by capitals of cast iron. (For details, see plates.)

The towers were raised with no other falsework than that actually used in handling the material of each successive section. Before beginning to raise a tower, a floor of long timbers reaching from pier to pier and loose boards, was laid at the site of the tower ; on this floor was erected a frame-work 30 feet high, and composed of two bents, one on each side of the tower ; each bent consisted simply of two posts 48 feet apart and a cap 55 feet long, braced with planks across the corners. These bents were kept in an upright position by long inclined braces reaching from near the top of each post to the floor. Sets of falls were attached by slings to the projecting ends of the caps, and with these falls the lower lengths of the columns were lifted by a hoisting engine and placed in position, the transverse and longitudinal struts were then put in place and the diagonal ties put on, the longitudinal ties being temporarily attached by a hook and eye plate to the same pin with the tranverse ties. A gin pole 55 feet high, was then lashed to each column and these gin poles were used to transfer the floor and frame to the top of the lower section of tower now completed. The same operation was then repeated with the second length of columns, which were placed over the tenon plates of the lower length, and secured by the pins ; this done, the longitudinal ties were changed from their temporary connections to the permanent ones. When the second section of the tower was completed, the frame was used to raise the gin poles, which were lashed again to the columns and rested on the longitudinal struts. The floor and frame were then raised again, and the process repeated till the tower attained its full height.* Tower D, the last tower raised, weighing 277 000 pounds, was entirely erected in 11 days, one day only having been previously spent in erecting the staging for the first section.

The 50 feet spans are of simple design and offered no special difficulties in erection, as single sticks of timber were long enough to reach from bearing to bearing. The longer spans required more staging. For this purpose four combination Pratt trusses were built, the tops chords of which were made of four pieces of pine 4 × 10 inches, packed in pairs and sprung about 4 feet apart at the centre ; the bottom chord was of straight parallel eye bars, and the posts V shaped; the form of the top chord made the truss stiff without lateral bracing. These trusses were put together below and raised by block and falls to the bottom of the upper section of the towers where they were placed, resting upon the transverse struts, two

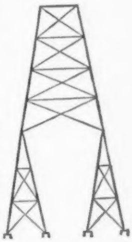
* This system of scaffolding was designed by Mr. J. H. Drake, superintendent of erection for the contractors, and did excellent service.

being used for each span. A suitable staging was then erected on them and the permanent truss was put together, the materials being run out from the end of the bridge. The first long span completed was that of 118 feet length, between towers E and F, the east 100 feet span being in progress at the same time. The false trusses were then lowered into the chasm, taken apart, removed, put together again, and made use of to raise the middle span between towers D and E which completed the bridge.

The trusses are of the simple Pratt pattern, with no other peculiarity than that they are made very narrow, the top chord being of I shape, formed of a plate and four angles. The laterals are attached to short vertical pins which pass through yokes fitting over the truss pins; these yokes are drilled with two sets of holes, only one of which are now used; the trusses are placed 19 feet 10 inches apart between centres; when a second track is to be put on, it is proposed to draw the trusses 8 inches nearer together, making the lateral attachment in the other set of pin-holes, and to raise another truss outside of each of the existing trusses. To give additional stiffness to so narrow a chord, a double set of laterals are used, attaching at the middle of each panel as well as to the pins. One end of each long truss is bolted to the iron capital of the column, and the other is placed on rollers, but connected with the next truss by iron loops passing over the end pins of each span, and which allow only the amount of motion needed for expansion. The short spans over the trusses are bolted to the capitals at both ends; the others are arranged in the same manner as the long spans. The end pins of the 50 feet spans are placed 6 inches from the centre of the columns, and those of the long spans only 3 inches, so that under a full load the centre of weight comes directly in the line of the centre of the column.

The plans of this viaduct were prepared in the hurry of a pressing necessity, and were obliged to conform in a measure to the plan of the original timber structure. Had there been no masonry already standing, it would have been preferred to place the two bents of each tower only 25 or 30 feet apart, and so avoid the unusual length of the longitudinal struts. The main principle of the plan may be said to be that which characterizes all American bridge building, and is the leading difference between the works of American and European engineers in this department; the concentration of the material into the least possible number of parts, a principle whose advantages are believed to be even greater in large and lofty viaducts of the class of the Portage bridge, than in the construction of trusses of long spans, to which it has been so generally and successfully applied. In the towers of the new Portage bridge, the

supporting material is all concentrated in 4 heavy posts, one at each corner, the mass of these posts giving greater stiffness than could be obtained from the same quantity of material distributed among several smaller members, and at the same time offering a minimum wind surface. The apparent objection of the concentration of a large weight on a single point is less than would at first appear ; the greatest weight which will be thrown on the base of any one column of the Portage bridge when completed to form a double track structure, will be 357 500 pounds, which distributed over the base of the pedestal stone, 4 feet square, amounts to 155 pounds per square inch, a pressure much less than is found under the bearings of many truss bridges. The minimum thickness of the Portage piers below the coping is 10 feet, and allowing for an equal distribution of strain by the masonry in all directions, the pressure imposed by the structure and load on a surface 10 feet square is only 24.8 pounds per square inch. With great heights, however, the side batter of posts necessitates the use of very long transverse struts in the lower sections of the towers, which are objectionable, requiring at Portage, intermediate vertical posts and lateral bracing to stiffen them, while the expansion and contraction of the iron becomes very considerable. These difficulties would be avoided by building towers of the form shown in the marginal sketch, the lower portion being formed of two independent towers, the columns of which are brought together at the top, and which will have a maximum lateral stiffness with a minimum of diagonal bracing; these two towers could be placed in their turn on three lower towers, and in this manner a tower of indefinite height could be erected without the use of a single long compression member.



The floor of the new Portage bridge is of timber and designed to give the track an elastic bearing which will relieve the iron from the impacts of suddenly applied weights. It is formed of oak timbers 8 × 14 inches, 22 feet long, placed only 10 inches apart and resting on the chords of the trusses. Two ribbons of pine, 9 × 9 inches, sized to 8 inches, are placed 10 feet apart, bolted with inch bolts to every floor timber, and serve to distribute the weight thrown on any one beam, over the several adjoining ones. The rails are laid directly on the oak timbers. A foot-walk of light plank is laid outside of each ribbon, and a substantial railing of wood completes the structure. During the passage of a train, a person on the foot-walk notices a slight tremor, due to the spring of the floor timbers, but in the towers very little vibration is felt.

The top surface of the river piers was badly shattered by the fire, and the lower courses of stone were badly broken by the action of frost and water, the stone of which they were built not being of the best character. In placing the pedestals, the broken upper stones were removed and a good bearing secured. As a protection to the masonry, the entire upper surface of the piers was covered with a layer of beton coignet. The lower courses of the piers, which rest on the rock bottom of the river, were enclosed in cribs of sawed oak timber, placed 18 inches from the stone, and the space between the timber and stone-work filled with beton well rammed.

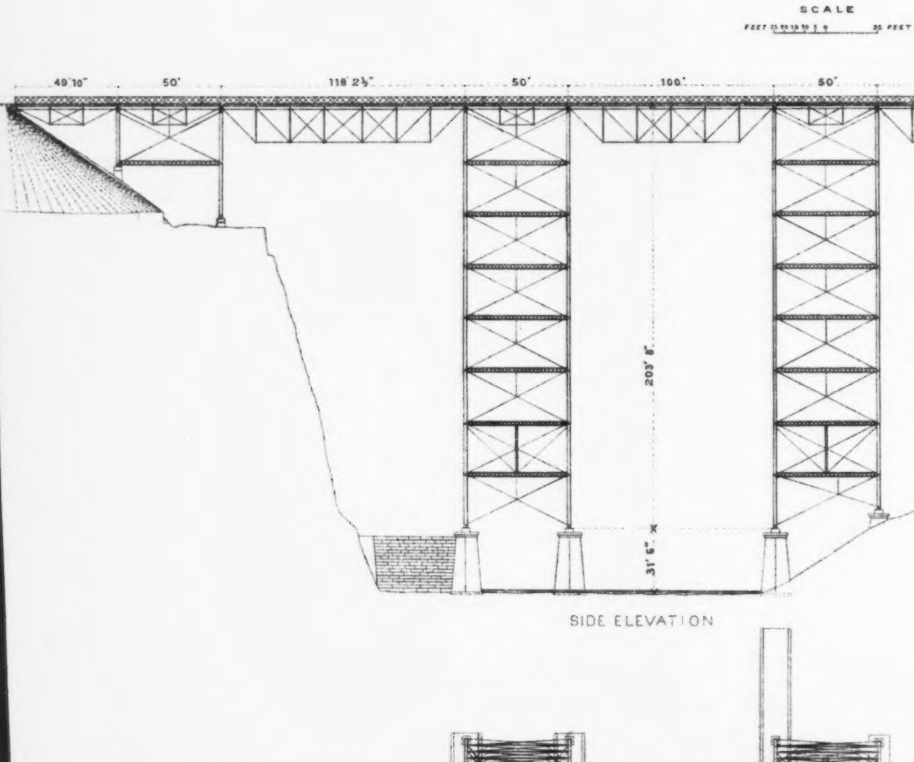
The total weight of iron in the bridge is 1 310 000 pounds, divided as follows:—

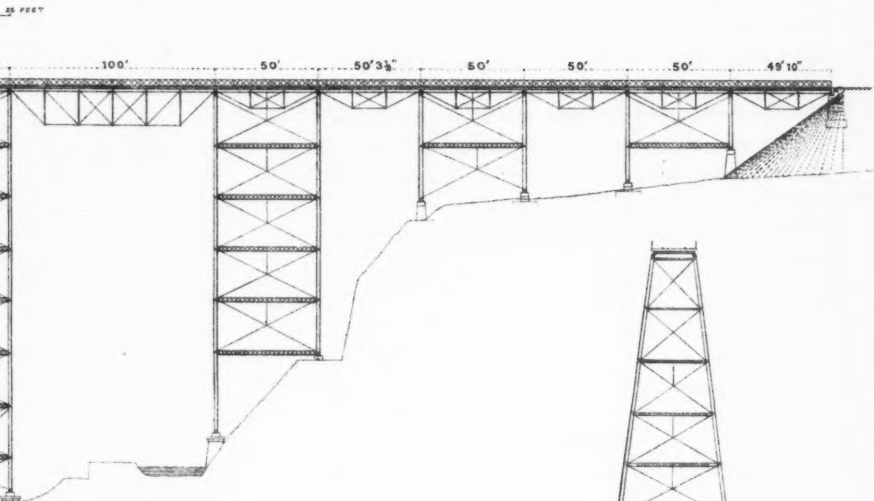
	POUNDS.	POUNDS.
Tower A	43 860	
" B	57 867	
" C	185 048	
" D	277 890	
" E	284 486	
" F	48 399	
Total Weight of Towers		897 550
10 Spans, 50 feet each	197 420	
2 " 100 " "	128 910	
1 " 118 " "	86 120	
Total Weight of Superstructure		412 450
Total		1 310 000

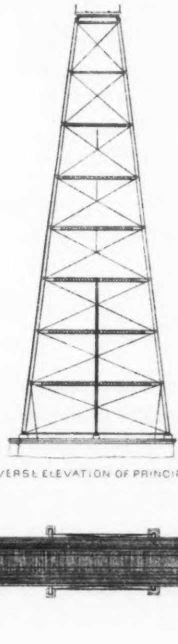
The amount of timber in the floor, foot-ways and railing is 112 318 feet of oak and 18 300 feet of pine (B. M.), while 27 987 pounds of iron were used in bolts and washers, the heavier bolts being all bolts taken from the ruins of the old bridge.

The total cost of the iron work was $87 973, this being for the structure erected complete, but including no transportation of manufactured material or painting after erection. The oak floor, with hand-rail and foot-ways complete, cost $6 200, and the painting (one coat only), $1 200; so that cost of the entire structure above masonry, did not exceed $95 000, a striking example of the present low price of iron work and the economy of American skeleton structures.*

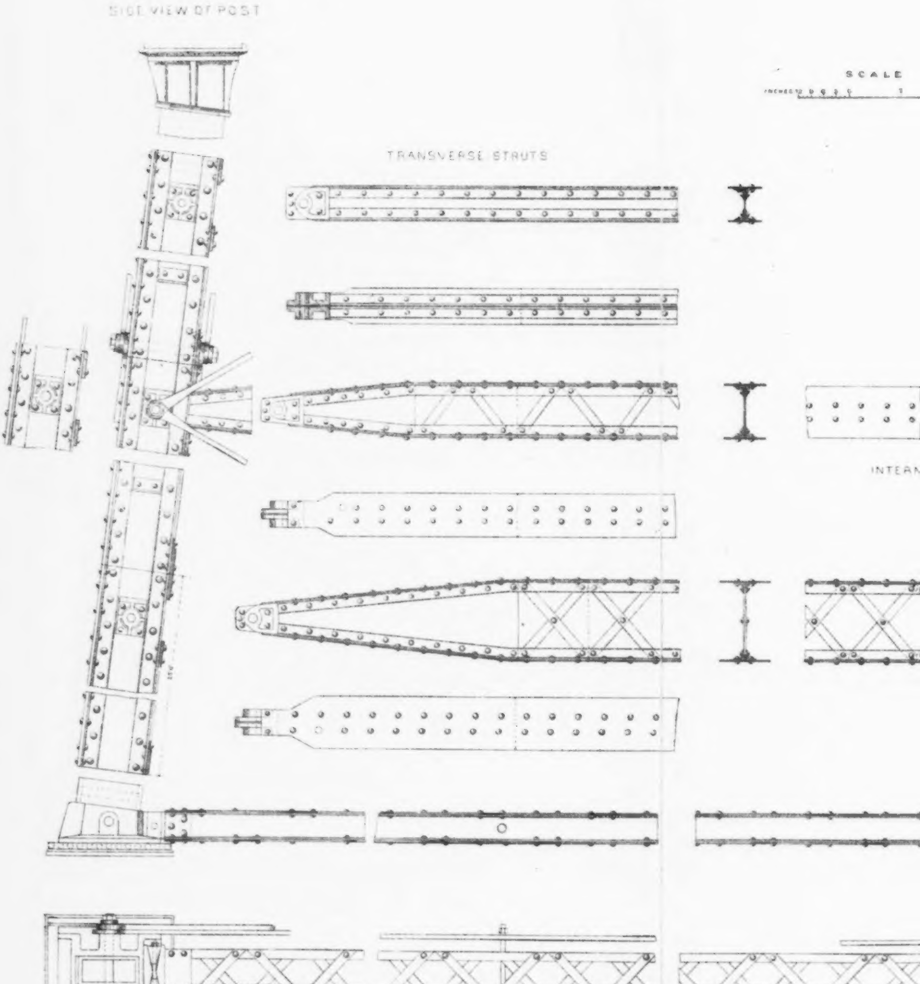
* The original structure was begun July 1st, 1851, and completed August 14th, 1852; it contained 1 602 000 feet (B. M.) of timber, and 108 862 pounds of iron; in the foundations were 9 200 cubic yards of masonry. The entire cost was about $140 000 (G. L.).


SCALE
FEET
25 FEET
49'10"
50'
118' 2½"
50'
100'
50'
203' 8"
31' 6"
SIDE ELEVATION
F
E
D
GENERAL PLAN

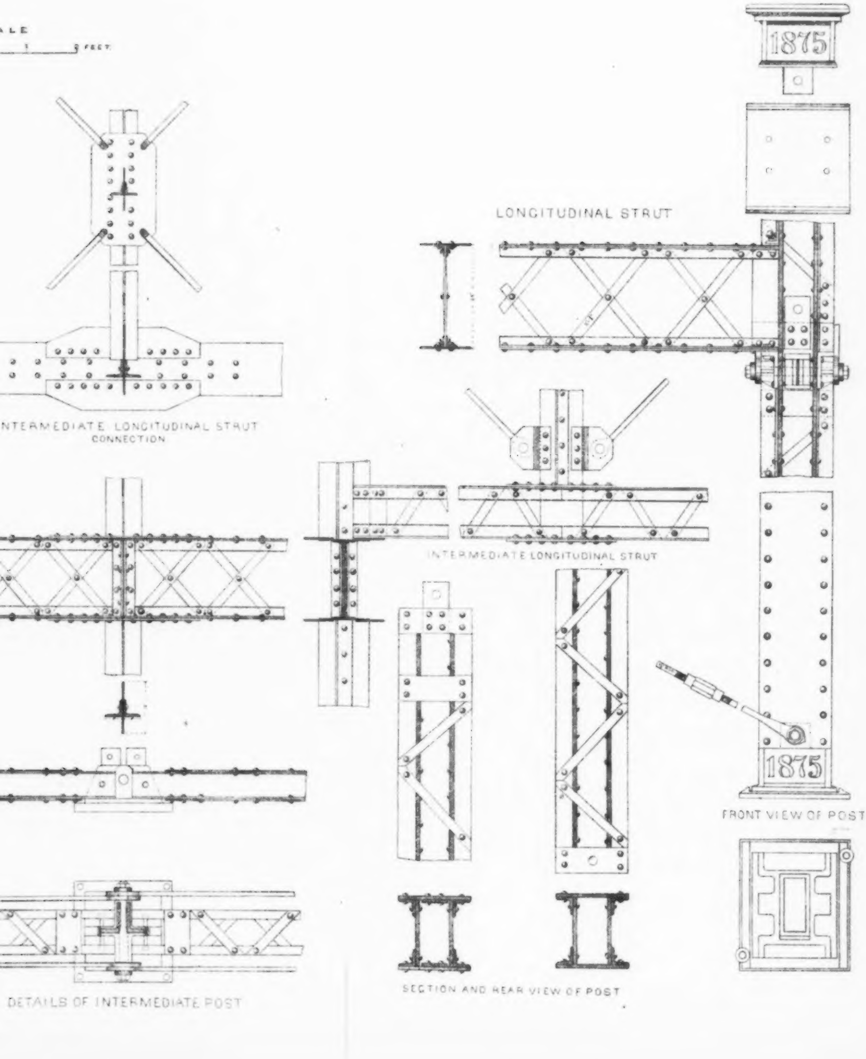


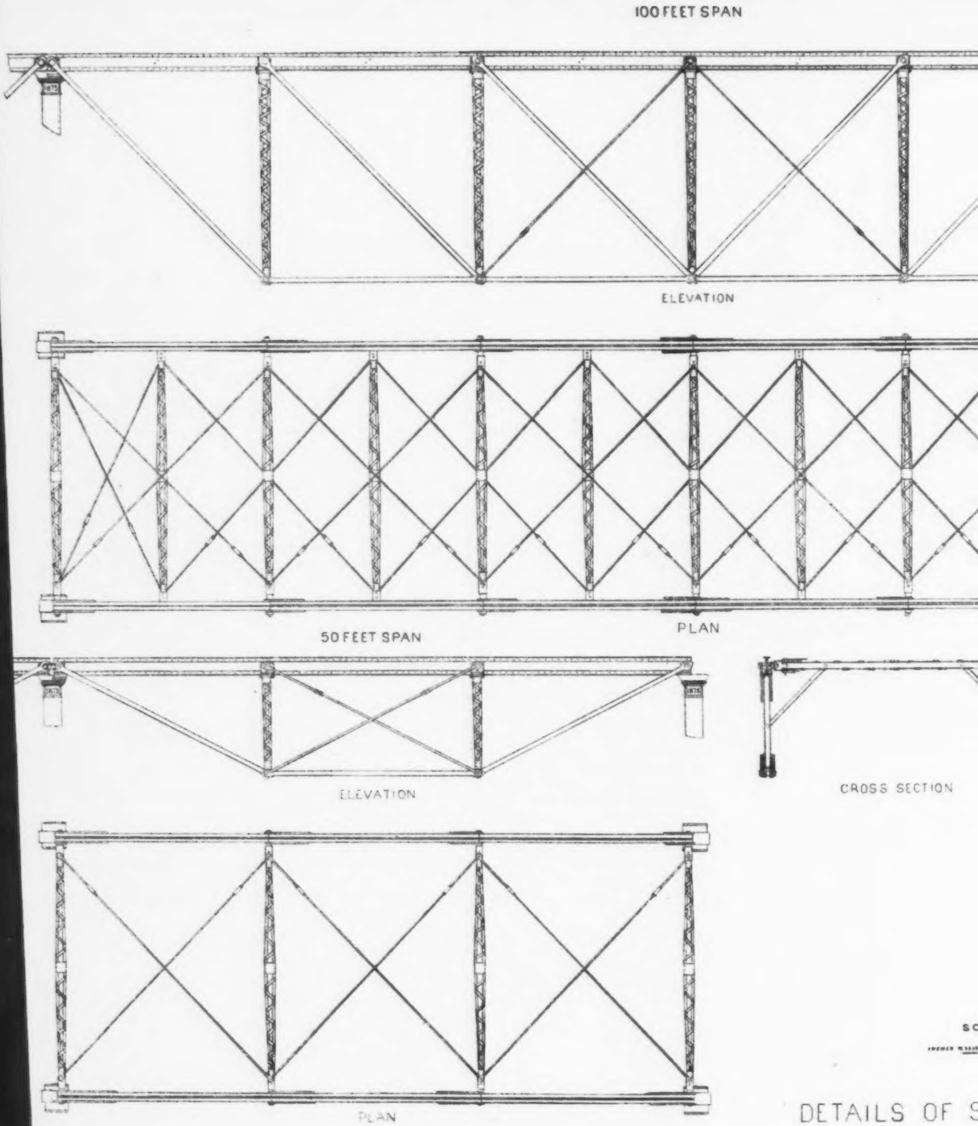


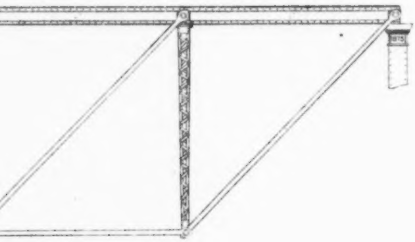
TRANSVERSE ELEVATION OF PRINCIPAL TOWER

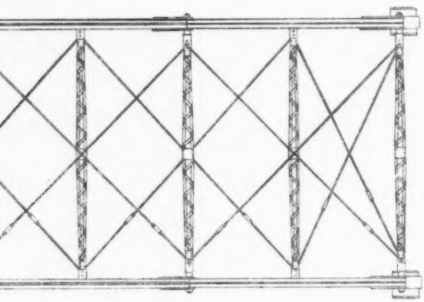

SIDE VIEW OF POST
SCALE
TRANSVERSE STRUTS
BOTTOM TRANSVERSE STRUT

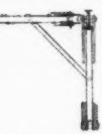
N°2


ALE
1 2 FEET
INTERMEDIATE LONGITUDINAL STRUT
CONNECTION
LONGITUDINAL STRUT
1875
INTERMEDIATE LONGITUDINAL STRUT
1875
FRONT VIEW OF POST
DETAILS OF INTERMEDIATE POST
SECTION AND REAR VIEW OF POST

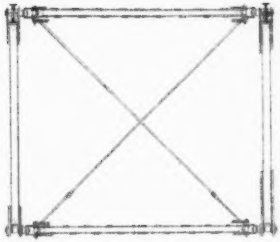

100 FEET SPAN
ELEVATION
PLAN
50 FEET SPAN
CROSS SECTION
ELEVATION
PLAN
SC
DETAILS OF S





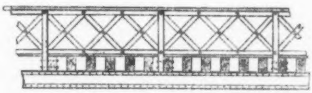


ON

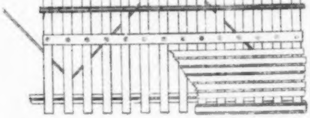


CROSS SECTION

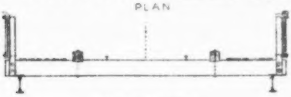
DETAILS OF FLOOR



SIDE ELEVATION



PLAN



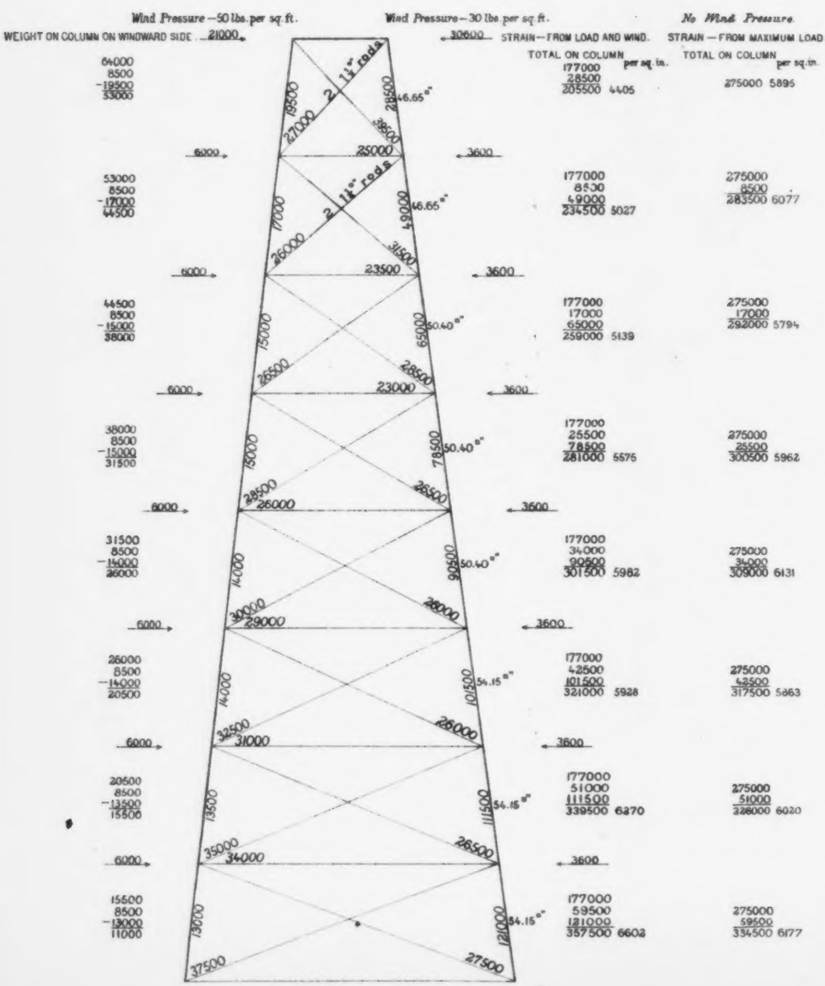
CROSS SECTION

SCALE

OF SUPERSTRUCTURE

STRAIN SHEET OF PRINCIPAL TOWERS.

SCALE



CXVII.

THE STRENGTH AND OTHER PROPERTIES OF MATERIALS OF CONSTRUCTION,

AS DEDUCED FROM STRAIN DIAGRAMS AUTOMATICALLY PRODUCED BY

THE AUTOGRAPHIC RECORDING TESTING MACHINE.

A Paper by Prof. Robert H. Thurston, Member of the Society.

Presented December 31st, 1875.

In a paper read before the Society in February and April, 1874,* the writer gave an account of a series of researches which he had made with a novel form of apparatus, and illustrated the work by *fac-similies* of a collection of automatically produced strain-diagrams. The new method of investigation adopted and the importance of some of the conclusions deduced from the autographic records have attracted much attention and the paper has been extensively republished.† It has recently been translated into the German for Dingler's Polytechnisches Journal, and its publication has been followed by a paper by a distinguished colleague of the writer, Prof. Kick,‡ of the Institute of Technology at Prague, who makes a number of criticisms§ which indicate that it may be advisable to consider some of the more obscure points in the original paper at greater length and to exhibit the sources of the errors which have been committed by the critic.

The first criticism made by Prof. Kick, as will be seen by a perusal of the paper, a translation of which is herewith given,‖ is a statement that important discrepancies exist between the results obtained experimentally

* Transactions, Vol. II, page 349; Vol. III, page 1; † Journal of the Franklin Institute, 1874; Van Nostrand's Mag., 1874; Dingler's Polytechnic Journal, etc., etc., 1875; ‡ International Jury, Vienna, 1873; § Kritik Uber R. H. Thurston's untersuchen uber festickeit und elasticitat der constructions; Materialen Von Friedrich Kick, Bd. 218, H. 3.

‖ Criticism of R. H. Thurston's "Researches on the Strength and Elasticity of the Materials of Construction," by Friedrich Kick.—Translated from Dingler's Polytechnisches Journal; Band 218, H. 8.

The results of Thurston's investigations of the strength and elasticity of materials, no less than the ingenious deductions therefrom, require the more thorough examination because of the important discrepancies arising between those results and the experiments instituted by myself for the determination of the relations of tensile and compressive forces as influenced by changes of form.

by the author of the criticism and by the writer. This difference is attributed to an assumed peculiarity of the apparatus and of the method of experiment adopted by the writer, which is asserted to produce serious errors. That such a difference does appear between the results obtained by the writer and the critic is undoubtedly the fact ; that they are attributable to the cause assigned is less evident, and what follows may prove the assertion entirely unfounded. The critic makes an assumption of faulty manipulation without evidence of its existence and then claims to "prove" mathematically that the apparatus, which is asserted to be "dynamic" in its action, records its results statically and thus introduces fatal errors of record.

The mathematical portion of the paper is correct, and we will take advantage of that fact and will show how far the adverse element—the resistance due to the acceleration of weight—which is so boldly asserted to be the cause of "serious" errors, is likely to introduce such errors.

Taking an extreme case, supposing a perfectly rigid test-piece to be under test, the velocity of motion of the weight would be precisely equal to that of the handle and would be a *maximum*. Actually, the test-piece always yields and the velocity of the weight is invariably less than that of the handle. In the greater number of cases, the weight moves with much less rapidity than the handle, even when moving at its highest rate of speed, and during the greater part of the test, the rate of motion of the weight is so low as to be imperceivable and incapable of measurement, and at other times, the weight actually moves slowly downwards, as is seen by reference to the published strain-diagrams, on which the relative motion of weight and handle can be readily determined.

The motion of the weight is, in fact, independent of that of the handle and varies with the resistance of the test-piece, rising or falling as that resistance increases or diminishes, always slowly and almost invariably

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The study of Thurston's treatise indicated that a certain error, sometimes insignificant, sometimes important, was inherent in the results, the source of which was to be found in Thurston's testing machine. This machine, acting dynamically, records its results as if they were statical, the greater the velocity adopted, the greater is this error. This assertion is proven as follows :

If a weight be suspended by a spring, when equilibrium occurs—whether for rest or motion—the tension of the spring is proportional to the weight. As soon, however, as acceleration takes place, in the case of transition from rest to motion, as well as with varying velocities during motion, the tension on the spring must change; this variation may be expressed by the variation of S in expression

$$S = G + \frac{v\,G}{g\,t};$$

for uniformly accelerated motion, in which S is the tension on the spring ; v is the velocity at the end of the time t, and g is the acceleration of gravity $= 9.808$ M.

with very much less velocity than that of the handle. In making tests with this machine, the handle is always moved very slowly, and when attempting to secure diagrams for scientific purposes especial precaution is taken.

The following figures represent the rate of motion of the *handle*, measured alternately for a somewhat rapid and for an ordinarily slow motion. The motion of the weight is, as has been shown, very much slower.

	Time.	Angle.	R. Cos.	Space.	Max. Moment.
(*A*)	1 Min.	16°.00.	47.125 in.	13.54 in.	135.987 ft. lbs.
			1.197 m.	0.362 m.	
(*B*)	2 Min.	37°.66	38.75 in.	32.00 in.	292.755 "
			0.984 m.	0.813 m.	
(*C*)	1 Min.	16°.00	47.125 in.	18.54 in.	135.987 "
			1.197 m.	0.362 m.	
(*D*)	1 Min.	37°.06	39.00 in.	31.70 in.	291.70 "
			0.996 m.	0.805 m.	

Where the effort was made to attain greater rapidity of motion, the following results were obtained:

	Time.	Angle.	R. Cos.	Space.	Max. Moment.
(*E*)	1 Min.	33°.66	40.75 in.	28.78 in.	267.02 ft. lbs.
			1.035 m.	0.761 m.	
(*F*)	1 Min.	47°.66	33.00 in.	40.63 in.	350.98 " "
			0.838 m.	1.032 m.	

Let $t = 1$ second, and let r have the following successive values for the corresponding values of S, and the value of G will be :

r.			S.			G.		
0.1 M.	0.4	2.0	0.05 M.	0.20	1.00	1.01	1.04	1.20
0.2	0.5	3.0	0.10	0.25	1.50	1.02	1.05	1.30
0.3	1.0	4.0	0.15	0.50	2.00	1.03	1.10	1.40

Thus, even when the increase due to uniform acceleration amounts to but 1 M. per second, the tension will be 20 per cent. greater than in the case of equilibrium.

Let us apply this investigation to the Thurston machine in which tests are made by torsion. By depressing the lever *C*, *B* rises, the axes of both being united by the test-piece lying in the jaws. (For description of the machine, see Transactions, Vol. II, page 350, &c., *Translator.*)

We may liken this apparatus to a balance to which has been added an automatic recording apparatus which records the constantly increasing pressures exerted upon the lever *C*, and which are transmitted to the weighted lever *B* by means of the test-piece. These records can only be based upon statical laws ; for Thurston actuates a movable recording-pencil, attached to the weighted arm by a fixed and invariable guide-curve which can only be constructed by reference to the statical moments of the weight *D*.

Neglecting unavoidable sources of error which are present in all automatic recording apparatus, this has one great defect.

Prof. Kick states correctly the resistance due to acceleration of the motion of the weight as equal to $\frac{r\,G}{g\,t}$, and the total amount of stress as

$$S = G + \frac{r\,G}{g\,t} \dots\dots\dots\dots\dots\dots\dots (1)$$

in which expression, $S =$ the total stress, $r =$ the acquired velocity at the end of the time t, $G =$ the weight and $g =$ the acceleration of gravity $= 32\frac{1}{6}$ feet $= 386$ in. $= 9.8$ m.

$$\frac{S}{G} = 1 + \frac{r}{g\,t} \dots\dots\dots\dots\dots\dots\dots (2)$$

Then, for the several cases just given, assuming the velocities to be those of the weight, as improperly asserted by Prof. Kick, we get:

$$\text{(A.)}\quad \frac{S}{G} = 1 + \frac{r}{g\,t} = 1 + \frac{13.54 \times 2}{386 \times 60} = 1.001212;$$

$$\text{(B.)}\quad \frac{S}{G} = 1 + \frac{32 \times 2}{386 \times 120} = 1.001401;$$

$$\text{(C.)}\quad \frac{S}{G} = 1 + \frac{18.54 \times 2}{386 \times 60} = 1.001212;$$

$$\text{(D.)}\quad \frac{S}{G} = 1 + \frac{31.70 \times 2}{386 \times 120} = 1.001347;$$

And for those cases in which the rate of acceleration was made as great as could be obtained by the exertion of all the strength of the operator:

$$\text{(E.)}\quad \frac{S}{G} = 1 + \frac{28.78 \times 2}{386 \times 60} = 1.002481;$$

$$\text{(F.)}\quad \frac{S}{G} = 1 + \frac{40.68 \times 2}{386 \times 60} = 1.006301.$$

It is constructed upon the basis of statical relations. In using the machine, equilibrium does not exist, but motion. The diagrams must therefore deviate the more from the truth, the more suddenly and the more rapidly the lever C is moved. If, therefore, the greatest care is not taken in experimenting, using low velocities and a steady hand, the diagrams will be incorrect and entirely untrustworthy for strictly scientific researches. No proof is required to show that the same force is required to produce the same acceleration, whatever the position of the lever carrying the weight at the beginning of the acceleration.

The moment of the force producing acceleration must be added to, or subtracted from, the moment of the weight D, according to the direction of the motion. Only the latter is graphically recorded—the former is not. The error thus arising would be constant for a uniform acceleration and could be corrected by drawing a line parallel to the curve, were it possible to move the lever C with a uniform acceleration. But, since this cannot be done by hand, such a rectification of the graphical record of the Thurston machine cannot be made.

Further, we have: that the above-given quantity which is to be added or subtracted is relatively of greater influence, the less the torsional moment of the weight D; in other words, the errors of the diagram due to the movement of the weight are of most importance at the initial portion of the diagram, within the elastic limit.

It is possible that the peculiar forms of the diagrams 6, 10 and 85, Plate B (*Plate II, Vol. II, page* 378), which are convex to the axis of abscissas, are a consequence of the more rapid motion of the hand-lever during those experiments, and, as well as the irregularities of the lines of the diagrams, may be partly explained by this dynamic action of the machine.

When, therefore, Thurston says: "(1.) To determine the homogeneousness of the material, examine the form of the initial portion of the diagram between the starting point and the

It is seen from the above that the maximum possible errors, due to the cause assumed by the critic as the source of the discrepancies which he has found to exist between his work and the self-recorded results given by the autographic machine, are necessarily some fraction of one-eighth of one per cent. Every experienced investigator in this department of scientific research knows, however, that this limit of error falls far within the limit of variation of quality of every material of construction, even when nominally of the same grade. The criticism is therefore seen to have no practical weight.

Now, determining the relative motion of handle as given above, and of the weight, from the strain diagrams published, and taking wrought iron as the best illustrative example, it will be seen that, within the elastic limit, the error claimed to destroy the value of the data secured may possibly amount to 0.001, and that at the limit of elasticity even this error entirely disappears, since the weight there ceases rising. Beyond the limit of elasticity, the error is that due to a rise of the weight equal to an exceedingly minute fraction of the motion of the handle, and is so small that it would be quite impossible to detect it on the diagram by any method of measurement in use. The criticism of the distinguished author of this "*kritik*" is thus seen to be quite insufficient to account for the discrepancies noted by him. He is quite right in looking for the source of error in the machine—provided that the results of the writer are erroneous and those of Prof. Kick are right—for, in the former, the

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sudden change of direction which has been shown to indicate the elastic limit. Notice, also, the inclination from the vertical, and compare it with the inclination of the elasticity-line.

" A perfectly straight line, beneath the elastic limit, perfectly parallel with the elasticity-line, shows the metal to be homogeneous as to strain; *i. e.*, to be free from internal strains, such as are produced by irregular and rapid cooling, or by working too cold. Any variation from this line indicates the existence, and measures the amount of, strain" (*Vol. III, page* 2). The first sentence, in consequence of the inherent error in the apparatus, is, in general, incorrect.

An indisputable and convincing proof that Thurston's machine is inapplicable to scientific investigations is found in the diagrams 101 and, particularly, in 118, Plate C (*Plate III, Vol. III, page* 30). The diagrams show, at *b*, *c* and and at *b' c'*, that a rapid increase of velocity is followed by a rapid sinking of the line. This must necessarily occur ; for the pencil of the registering apparatus, in consequence of the peculiarity of this construction, does not record that moment which is exerted in the acceleration of the weight of those parts which are set in motion by the test-piece.

But Thurston deduces from these diagrams a direct reply to the question: what is the relation of the resistance of the test-piece to rapid or slow distortion ? He deduces, from the fall of the line of the diagram with the motion, that the resistance decreases as the velocity of strain increases. We have seen that this assertion is not confirmed by those diagrams. That Kirkaldy has reached the same conclusion may be due to a similar misinterpretation of the results of experiment. If Thurston's diagrams could give a definite answer to the question, they would rather read—the resistance is independent of the velocity of distortion.

Referring to the conclusions deduced by Thurston's incorrect method, it is remarkable to find it stated that : " To determine the resilience of the material within any assumed limit of

story is told by the machine itself and cannot be attributed to errors of personal observation as may those existing in data acquired by the older methods of research.

It may be safely asserted that the errors due to the inertia of the weight and to its acceleration may, by careful handling, be made as minute and as practically immeasurable as those due the same cause in the older forms of testing machines. Considering the facts, that the results obtained by the older methods of testing are liable to errors arising from personal observation, while, in the method adopted by the writer in the autographic recording testing-machine they are automatically registered, it would seem that the advantage, in respect to accuracy, must be on the side of the new method.

The writer believes the facts exhibited above to prove conclusively that the bold assertion of the foreign critic—that with the greatest care these strain-diagrams are liable to be incorrect and untrustworthy—is without real basis and is itself absolutely incorrect.

The second criticism of Prof. Kick, in which he suggests this assumed source of error to be the cause of the differences in the initial portions of diagrams (6, 101 and 85 of Plate II, Vol. II., page 378), which the writer attributed to peculiar conditions of molecular or mechanical structure, is not only invalidated by what has been shown above, but most conclusively by a large number of experiments made before and after the date of the original paper, in which the noted peculiarities were very marked, although the experiments were conducted with uniform precaution. The fallacy of the criticism is still further proven by the characteristic differences

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extension, measure the area of the curve up to the assumed limit. To determine the total resilience, or the shock-resisting power, measure the total area of the diagram;" (*Vol. III page* 3).

While, on the other hand, he concludes: "The rapidity of action, in the cases of shock, and where materials sustain live loads, is a very important element in the determination of their resisting power; not only for the reason given already, but because the more rapidly the metal is ruptured, the less is the resistance to rupture;" (*Vol. III, page* 15). Since the last sentences which contradicts the former is incorrect, the former *may* be correct; it remains, however still unproven.

Similarly unproven is the assertion: "The effect of repeated bending, or other form of strain, can be thus inferred from an examination of the strain diagram of the material, obtaining from a single experiment a determination hitherto only obtained by a long and tedious process of repeated distortion;" (*Vol. III, page* 8). It is here quietly presupposed that the diagram of resilience up to the point of fracture, for the test-piece strained by torsion, record, also the amount of work done in case of fracture for all other kinds of strain. The proof of this is wanting; it would be difficult to produce it.

[The critic forgets, throughout his paper, that the *quality of the material* is the property which it is attempted to determine, *not* the relations of the several kinds of strain.—R. H. T.]

Thurston mentions the fact that: "The phenomenon here discovered is an elevation of the limit of elasticity by a continued strain;" (*Vol. III, page* 12). This "discovery" has also,

noted in the initial portion of the diagram where different metals are compared, as shown in the published diagrams of iron, steel, copper, tin, etc. Such differences could not possibly arise from the assumed cause.

Professor Kick adduces as what he asserts to be "absolute proof" of the existence of the source of error above alluded to, the peculiar strain-diagrams, 101 and 118, Plate III. These show the rapid motion of the handle (not of the weight) to be followed by a fall of the weight and a drop of the pencil. This was attributed by the writer to a weakening of the metal by rapid distortion ; a conclusion which has been confirmed by a study of Kirkaldy's experiments with his tension apparatus, by many experiments since made by the writer with the autographic machine, by numerous experiments made by Com. Beardslee at the Washington Navy Yard with a tension machine having peculiar facilities for exhibiting this phenomenon, and especially by the experiments made on a very large scale on iron beams for targets, as described by Gen. Barnard in a paper read before the Society (Transactions, Vol. I, page 173), and referred to by the writer, in the discussion at the Seventh Annual Convention (Vol. III., page 128).

The error into which the critic has fallen will be seen at once when it is noted that during this rapid motion of the handle and the distortion of the test-piece, produced by a heavy blow on the handle, the weight had no time to move and the drop of the weight succeeded the distortion, as is explicitly stated in the original paper to be an evidence of the weakening which is a consequence of rapid distortion. This evidence would seem to be quite sufficient. But the experiment described by Gen.

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been made and published by General Uchatius (*Die Stahlbronze, Vortrag. Gehalten am 10 April*, 1874, *Wien.*) in 1873, in the following words : "In all metals which possess a considerable degree of ductility, it is interesting to extend the investigation to the elastic limit. We thus learn that these metals attain their highest elasticity only when they are strained beyond the elastic limit to a certain point and are allowed to remain so for a time." "This advantage has never, until now, been noted."

Thurston's machine tests specimens for torsion and the extension of the external fibres, calculated from the angle of torsion, cannot be considered as the measure of the extension of the fibres by tensile tests, for two reasons : *First*, because a diminution of length of the torsion test-piece must occur, which is not measurable from the angle of torsion graphically recorded by the machine. *Second*, for the reason that the external fibres are reinforced and strengthened by the internal fibres, in consequence of their lateral cohesion, they being less affected. That elongations of 69 and even 120 per cent. are found in wrought iron can only find its explanation on pages 100 and 101—elongations which no wrought iron will give.

If Thurston finds nothing remarkable in this, but states that this elongation of fibre is proportional to the reduction of section noted with the standard testing-machines, it should be said, on the contrary, that it is entirely inadmissible, for the percentage of elongation to be given any relation to the reduction of cross section. Robert Lane Haswell has noted this already. If we pull apart a test-piece like that shown in the accompanying illustration [The sketch represents a pair of test-pieces, of which one, *a b*, is intact ; the other, *a' b'*, on the

Barnard, to say nothing of those of Com. Beardslee, are certainly conclusively corroborative.

The exception taken by the critic to the principles (6) and (7) are fully met by the above and no more need be written on this point.

In the paper here referred to, Prof. Kick goes on to state that the phenomenon of "elevation of the elastic limit by strain," claimed to have been discovered by the writer, was discovered by Gen. Uchatius of Vienna, and published in April, 1874. (*Die Stahlbronge Vortrag. Gehalten am* 10 *April*, 1874, *Wien.*)

The writer is greatly pleased to find his work confirmed by so distinguished an authority, but his own discovery of this remarkable and important phenomenon was made months earlier, and was announced at the Annual Meeting of this Society, November, 1873, and formally placed on record in a "Note on the Resistance of Materials," read November 19th, 1873. (Transactions, vol. II, page 239.) The phenomonon was also discovered by Com. Breadslee, U. S. Navy, soon after, and by an entirely independent method of investigation, and was made known by him before the end of that year. It has since been observed by many experimenters, but the writer has as yet met with no claim of priority of discovery.

Prof. Kick asserts that the extensions estimated by the writer cannot be correct, because of a diminution of length in the specimen, and because of the influence of the cohesion existing between the inner and outer fibres of the mass. The writer can only say that experiment does not seem to confirm these assumptions and assertions.

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point of rupture at a point of largely reduced section.—*Translator.*] the greatest reduction of area will occur at the point of rupture, and at that point, also, is the greatest elongation.

If we determine the percentage, or the extension by taking the greatest length, *a b*, immediately before rupture, and designate the proportion of elongation by the quotient, $\frac{a'b'-ab}{a\,b}$, then, for the same material, very different values will be obtained according to the length *a b*, whereas, the amount of extension at the point of rupture, which is nearly the same in either long or short specimens, greatly changes the percentage of extension.

It is easily seen that a large percentage of extension will be obtained with very short test-pieces than with long ones of the same material. The reduction of area, therefore, affords a means of measuring the ductility of the material, affording, however, no precise determination of the percentage of elongation, which can only have a definite value when taken within the elastic limit.

The theory of strength of materials is a department of mechanics in which the greatest care should be exercised in drawing conclusions; it would also seem to be better to admit this, where satisfactory results are not obtained, than to enter with indefinite phrases into the realms of conjecture.

In that part in which Thurston treats of the effect of temperature upon the resistance of materials, conclusions 1 to 9 (*Vol. III, page* 21) have no significance, and simply say, "We do not know what is determined." The following sentences are not more valuable: (10.) That

In regard to the elongations given by the writer, amounting, with some ductile materials, to 120 per cent., it need only be repeated that it was explicitly stated that those figures are given as the best indication of the ductile quality of the material, that they are proportional to the maximum elongation of the most extended portions of the metal tested by tension, for the very reason stated in opposition by Prof. Kick, that the tension specimen invariably "necks down," and does not stretch as a whole, or uniformly; and it was stated that these factors of extension are related to the reduction of cross-section observed in tension, and are such as do occur within the elastic limit in homogeneous materials and such as would be observed were the material under tension, to draw down uniformly from end to end until fracture occurs, leaving the whole piece, in that case, of the diameter of the fractured section actually observed in the tension experiments.

The writer has stated his idea that the reduction of section by tension and not the extension of the whole specimen, is the most accurate measure of the ductility of the material. After passing the elastic limit, and after "necking down" begins, the elongation of a test-piece under tension is a function of its diameter and not of its length; and the whole extension may be expressed by the formula, $E = A\,l + B\int d$, an expression which the writer has not yet met with in any work on this subject.

The writer has noted these errors of the critic with as much surprise as regret, and especially as he finds them associated with the very excellent caution against "roaming in the fields of conjecture" in such scientific work.

Finally, comparing conclusions (10) and (11) with (19) and (20?) in which the effects of temperature are referred to, the critic notes an apparent

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the general effect of increase or decrease of temperature is, with solid bodies, to decrease or increase their power of resistance to rupture, or to change of form and their capability of sustaining dead loads. (11.) That the general effect of change of temperature is to produce change of ductility, and, consequently, change of resilience or power of resisting shocks and of carrying live loads. This change is usually opposite in direction, and greater in degree than the variation simultaneously occurring in tenacity. On the other hand: (19.) In pure and well-worked metals, decrease of temperature is accompanied by an increase of strength as well as an increase of elasticity and resilience. The last statement is evidently contradictory of conclusion 11. Which is now correct?

With the great care which we know to have been taken in the translation, these inconsistencies must be from the original, which we have not at hand. We are the more certain of this from the fact that other discrepancies exist, which, from appearances, could only have been transferred from the original, although they are of comparative insignificance.

In these opposing statements, the value of the Thurston machine is not contested for *practical* purposes. In many cases the diagrams recorded by this undeniably simple apparatus have contributed to the confirmation of tests of resistance of materials, and the merit of Thurston is decided and indisputed.

discrepancy which a more careful reading would have explained and the necessity of reference to them, perhaps, not have arisen. It is not, however, impossible that the writer was not sufficiently explicit. Referring to the original paper, it will be seen that the author quotes from an earlier monograph on the effects of temperature in which all of the earlier researches of both physicists and engineers, so far as they were accessible to him, were collated, and the conclusions, derived by comparison, were that a rise of temperature decreases the resisting power of materials while increasing their ductility and sometimes their resilience; a decrease of temperature seemed to produce the opposite effect. The generally conflicting testimony of those who, on the one hand, had experimented by steady stress, and those who, on the other hand, had experimented by shock, thus seemed to be reconcilable. The apparent discrepancies between authorities were concluded to be due to differences of method similar to those which are claimed by Prof. Kick to distinguish the researches of the writer from those of the better known authorities, —but with more reason.

Subsequently the invention of the autographic testing machine having, for the first time, furnished a means of making simultaneous determinations of the several mechanical properties of the test-piece, the real facts seemed to be proven to be slightly different, and as stated in (20) that with pure well-worked metals the principle enunciated in (28) is fully illustrated, and a decrease of temperature is accompanied by an increase of strength, ductility and resilience; (21) that materials which are impure and irregular in character may exhibit exceptions to and even reversals of that principle in changes of ductility, and, while increasing in power of resisting simple stress, may, by a diminution of temperature, lose their power of resisting shocks; and that the effect of change of temperature probably varies with the character of the material.

The writer is grateful for the pleasant compliment contained in the closing paragraph of the paper of Prof. Kick, and trusts that the above remarks may indicate that the ordinarily useful work of the confessedly valuable addition to "practical" testing apparatus, which has been found in the autographic recording testing machine may prove to be supplemented by not less valuable scientific work.

AMERICAN SOCIETY OF CIVIL ENGINEERS.

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TRANSACTIONS.

NOTE.—This Society is not responsible, as a body, for the facts and opinions advanced in any of its publications.

CXVIII.

DESCRIPTION AND RESULTS OF

HYDRAULIC EXPERIMENTS WITH LARGE APERTURES,

AT HOLYOKE, MASS., IN 1874.

A Paper by THEODORE G. ELLIS, C. E., Member of the Society.

FOR WHICH THE NORMAN MEDAL WAS AWARDED

NOVEMBER 3D, 1875.

INTRODUCTION.—In the summer of 1874, some experiments were tried under the direction of the writer, to determine the volume of water discharged from large orifices under different heads. The trial originated in making a practical test of the amount flowing through an aperture of 1 foot square, under a head of 2 feet, to determine the number of cubic feet per second that could be used by certain mills, under an old contract based upon that quantity as a unit.

The place chosen for the experiment presented so many advantages for the purpose, that, in the absence of any published experiments with large square-edged apertures, and in view of the need that was felt among hydraulic engineers for some better data and more reliable coefficients than those derived from the experiments of Poncelet and Lebros —whose largest aperture was 8×8 French inches, or a little more than $\frac{5}{12}$ English foot in area, and whose greatest head was 10 feet—a series of carefully conducted experiments was undertaken to determine coefficients for as large orifices as could be conveniently tried at this place.

The liberal offer of Mr. Stephen Holman* to defray the expenses and furnish the apparatus for these experiments, at once determined the writer to undertake them, and the kind tender of the use of his flume

* Fellow of the Society, and President of the Holyoke Machine Co.

by Mr. James Emerson, who had at considerable expense fitted up one of the large locks of the Holyoke Water Power Co. for the purpose of testing turbines, furnished perhaps as desirable a place for the purpose as could be found in the country.

The writer also had the valuable co-operation and assistance of Mr. N. H. Whitten,* who superintended the construction of the plates with which the experiments were tried, with the exception of one, of 1 foot square, which was made at the Colt's Patent Fire-Arms & Manufacturing Co.'s works at Hartford, for Mr. James L. Cowles of Farmington, and kindly loaned by him for these experiments. Mr. Whitten also superintended the constructions and alterations at the flume required by the experiments, and assisted in taking the observations. He read in all cases the gauges indicating the head of water in the flume, while the writer read the gauge at the measuring weir.

Description of the Place.—The lock between the upper canal and the next below it, in which the experiments were tried, is 100 feet long, 16 feet wide, and 20 feet lift. It is built of cut stone, and is furnished with wooden gates of ordinary construction. It had been fitted up by Mr. Emerson as a testing flume for turbines, by constructing a strong bulk-head, about 50 feet above the lower gates, reaching from above the level of the water in the upper canal down to within about 5 feet of the bottom of the lock. From this point a horizontal floor extended back about 10 feet, supported by posts, from which point the bulk-head again continued downward to the bottom of the lock. The general construction of this bulk-head will be seen by reference to the plan and vertical section of the lock, Fig's 1 and 2. In the horizontal flooring was an opening through which the water flowing from the turbines passed to the reach below. This bulk-head divided the lock into two chambers, the upper one of which, when the opening in the floor was closed, was as nearly as practicable water-tight.

Below the bulk-head was a basin 48½ feet long, at the lower end of which was a measuring weir in a partition or dam across the lock from side to side. The crest of this weir was about 6 inches above the ordinary level of the water in the lower canal, and the partitions at its ends extended 2½ feet higher.

Over the upper part of the lock, extending from above the upper gates to some distance below the bulk-head, was a building containing the necessary space for the workshop and machinery required in testing

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* Engineer of the Holyoke Machine Co.

turbines. The entrance was from the level of the top of the lock into a room over the horizontal floor of the bulkhead before described. In this room was an open hatchway, directly over this floor, which gave easy access to the upper side of the vertical part of the bulk-head. About half way down to the bottom of the flume was another flooring with a similar hatchway, and a stairway reached down through these two stories from the room above. Over the hatchway was a traveling crab, for lifting and placing machinery in the above described well.

The lower part of the lock, including the weir, was roofed over within the walls at a lower level than the roof of the building above, and a stairway led down from the room over the hatchway above the bulk-head, to a platform, extending the whole length of the basin below, from the bulk-head to the weir, and which was continued a short distance below the weir by descending a few steps to a lower level.

Across the weir basin, at intervals of about 8 feet, were cross-timbers, 6 × 6 inches, reaching from side to side, and supported by posts from the bottom, of the same dimensions, 2 feet from the sides of the lock. The top of these timbers was about 2 feet 3 inches above the crest of the weir, and upon them rested the platform and planks for access to all parts of the basin. There were five of these framings across the basin, dividing it into six bays. The upper one had a square timber across the lock, excepting about 2 feet at each end. This was under water, at a depth of about 1 foot below the weir. The cross-timbers supported a part of the posts of the roof framing, while others extended down the sides of the lock to the bottom.

The lock had a plank bottom, 6 inches thick, at a depth of about 6½ feet below the crest of the weir; but there was an average of about 1 foot of mud, stones and rubbish, above this bottom. The depth was 5½ feet at the weir and 6½ feet at the farther end.

It will be seen that this place offered more than ordinary advantages for the trial of experiments upon the volume of discharge from large apertures, the only limit being the amount of water that could be accurately measured over the weir at the lower end of the basin.

The Apparatus Used.—Previous to commencing the experiments, the opening in the bottom flooring of the flume was closed by covering it with 3-inch planks, spiked down to the flooring. An opening was cut in the vertical part of the bulk-head at a suitable distance above the level of the weir basin, and a frame placed around it so as to form a firm bearing for the plates containing the experimental apertures. (See Fig's 3 and 4.)

Upon the inside of the flume, a gate or cover was hinged at some distance above the aperture, upon a horizontal bar, so as to open upwards, for the purpose of opening and closing the apertures upon which experiments were to be tried. This gate was provided with a loop to which a chain was attached when in use, reaching to the hook of the hoisting crab above the hatchway. By this means it could be opened, even under considerable pressure.

This was depended upon for discharging the water from the flume after it had been filled, with the aperture closed, to determine the leakage; but the first attempt to open it under about 17 feet head, with a 2×2 feet aperture, was but a partial success. After being partly opened, the bar forming the hinge bent under the pressure caused by the rush of water through the opening, causing also the chain to fail, so that the cover fell back against the opening; but fortunately being askew, it did not entirely close the aperture, and allowed the water to discharge. No detention to the experiments was occasioned, as its service in closing the aperture for the time had been performed. When the water fell it was removed, and in the succeeding experiments it was strengthened so as to withstand the pressure upon its hinge. For the smaller apertures, of 1 foot square and under, a cover without hinges, (see Fig's 5 and 6,) was used in the experiments to determine the leakage, and was removed by a chain attached to the hoisting crab above, in a similar manner to that of larger apertures.

About 10 feet back from the bulk-head was a plank screen, $5\frac{1}{2}$ feet high, bored full of inch holes, so that at the lower heads, whatever current might exist from the upper to the lower end of the chamber, should be distributed and equalized. The large size of the channel in proportion to the area of the apertures, however, rendered the current inappreciable.

There were two gates by which water was admitted to the lock, one was a small opening in the large upper lock gate, by which small quantities could be let into the lock chamber. The water entered a spout, (Fig's 1 and 2,) by which it was conducted vertically downward to below the level of the surface in flume and created no current or disturbance. The other was a large submerged gate in the masonry of the lock, which was opened and closed by a screw and geering from the top. This was generally used for regulating the heads in the experiments. The water from the sluice leading from this gate entered through the bottom of the lock, as shown in the drawings, some distance below the apertures experimented upon, in such a manner as to produce no current of approach, and no apparent disturbance near the place of discharge. The entrance of water from these gates created no appreciable current

towards the apertures. The flume chamber was 34.5 × 16 feet, furnishing a sufficiently large reservoir to prevent any sensible velocity of approach to the outlet.

Below the flume chamber was the weir basin, 48.52 feet in length, and 15.83 feet average width. In order to equalize the flow in this basin, and to prevent, in a measure, the commotion resulting from the falling into it of the jet from the aperture, the following obstructions were placed in it before the experiments were commenced. In addition to the timbers and supports for the floor and roof, before named, there was a stop formed of horizontal planks 8 inches below the level of the weir, upon the posts which sustained the second floor beam from the weir.

As there existed some irregularity and motion in the surface of the water at the highest head tried with the 2 × 2 feet aperture, this arrangement was changed July 26th, after that experiment, when the water was drawn off from the lower canal for the purpose. The changes were :—

Across the posts of the first floor timber from the aperture a stop was placed, of 3-inch plank, extending from the bottom of the basin to within one foot of the level of the weir. Upon the second floor framing was placed a stop of 1½-inch plank, extending from 2.6 feet below the weir, up to the floor beam ; and upon the third floor framing was placed a stop of 2-inch plank, extending from the bottom of the basin up to within 1.4 feet of the level of the crest of the weir. Above the fifth floor framing, and resting against the posts, a floating plank was placed which could rise and fall with the surface of the water.

The first three stops or barricades, over two and under one of which the volume of water passed, were for the purpose of arresting and equalizing the great rush of the current in the middle and upper part of the basin, caused by the larger apertures. The first barricade of 3-inch plank, receiving almost the full force of the jet, was necessarily made much stronger than the others. The great commotion caused in the upper bay by the spouting water, became less and less in the successive bays until it passed quietly over the weir. The floating plank just above the weir proved very effective in smoothing the surface ; so that the undulations, and also the small waves caused by the spray, were entirely neutralized before reaching the weir.

The foregoing arrangement was continued in all the experiments with the larger apertures ; but some of the stop planks having been removed during an interval between the experiments—from July 27th to August 12th, when the flume was used for other purposes—the following changes and modifications were made in the succeeding experiments.

At the first frame from the bulk-head the planking was retained, except about one foot at the bottom, and was carried up to the floor beam, rather higher than it was before. All the rest of the stop planks were removed, except one plank across the bottom at the third frame from the bulk-head.

Floating planks having proved so effective in stilling the undulations in the weir basin caused by the jet, a float of one plank was placed at the second frame from the bulk-head, and floats of two planks each were placed at the fourth and fifth frames, or the two nearest the weir. These operated very successfully, and prevented all undulations in the subsequent experiments.

The measuring weir at the foot of this basin was a solid timber dam, composed of 3×14 inch planks, extending across the width of the lock-chamber, and faced with 2-inch planks upon the upper side, and with 1-inch planks on the lower side, as shown in Figs. 1, 2 and 7, forming a dam 17 inches thick. This was finished at the top by being beveled off upon the lower side, as shown in Fig. 7.

The crest of the weir was formed of an iron bar, $5\frac{1}{2}$ inches wide and $\frac{1}{2}$-inch thick, planed true and bolted to the upper side of the planking of the dam. It reached from end to end, and was set perfectly level, remaining so during the experiments. The top of this bar was about an inch above the top of the planking.

At a height of 2 feet above the crest of the weir, a beam extended across the lock chamber, and a series of stop planks were provided which rested in a groove in the top of the dam and against the upper face of this beam, as shown in Fig. 7. These stop planks were each 1 foot wide, and were grooved so that a wooden strip fitted into the adjacent edges to make the joints water-tight when they were placed together. These planks rested against the face of the iron crest bar, and those whose edges formed the side of the weir were furnished with vertical strips of the same width as the crest bar, to form the side edges of the weir.

By means of these planks any desired length could be given to the weir, and its length could be changed with great facility whenever required. The lengths of weir used in these experiments were from 2 to 10 feet. This latter length, left 3 feet on each side between the end of the weir and the side of the basin, which was sufficient to give perfect contraction in all cases.

Just below the weir on the left, looking down stream, and reached from the platform above, was a small platform for the gauging apparatus. This was a little lower than the crest of the weir, and a small opening

through the planking, near the end, exposed the top of the crest bar, so that its level, with regard to the gauge, could be frequently tested.

Along the inside of the dam, at a depth of 3 feet 2½ inches below the crest of the weir, was a perforated copper pipe of 1-inch outside diameter, the end of which next to the platform was connected by means of a rubber tube, passing through the planking of the dam, with a bucket arranged with a pulley and weight, so that it could be easily raised and lowered to suit the level of the water within it. This would always be the same as that above the weir. It has been conclusively shown by the experiments of Mr. Francis at Lowell, that a pipe thus situated has upon it the pressure due to the full head upon the weir, no appreciable difference existing between the pressure just under the crest of the weir and that at a distance above, where the downward curve of the surface commences.

Above the suspended bucket, a hook-gauge, of improved construction, was firmly attached to the masonry, in such a manner, that, with the point of the hook at the surface of the water, the scale would range from zero, at the crest of the weir, to above the greatest depths used in the experiments.

A representation of this gauge is given in Fig. 16. Its operation is precisely like the gauge described by Mr. Francis, but it has some improvements which render it more convenient in use. The movable bucket, for showing the level of the water in the weir basin, is very much superior to using the gauge in a box above the dam. The observer has the level of the water brought directly in view, instead of being obliged to look down upon it from above, in generally an obscure light. In the present experiments a gas jet was placed so as to throw a proper light upon the surface of the water in the bucket, in order to observe the contact of the hook with the greatest exactness.

This gauge was provided with a clamp and screw motion of the whole frame, to adjust the index of the vernier to zero when the point of the hook was at the height of the weir. The screw which raised and lowered the gauge bar and hook, was placed by the side of the graduated bar and connected with it by an arm which could be geared into or detached from the screw at any point and clamped in its place. This permitted the hook to be raised or lowered rapidly, to any height, without turning the thread through the whole distance, as would be required with Mr. Francis's gauge.

The heights of water upon the apertures in the flume, up to 5 feet, were measured with a hook-gauge arranged with a movable bucket in the

same manner as at the weir, excepting that the rubber tube, connecting the bucket with the water inside, was inserted directly into the bulk-head. Above 5 feet, the heads were read upon a graduated rod attached to a glass tube which was connected by a rubber pipe with the water within the flume.

These gauges were all carefully compared with the levels of the weir and apertures as often as required to insure accuracy. The apertures experimented upon were, as near as could be constructed, as follows: In the vertical bulk-head;—2 by 2 feet, 2 by 1 feet, 2 feet by 6 inches, 2 feet diameter, 1 foot square, 1 foot diameter, and 6 inches diameter; and in the bottom of flume;—1 foot diameter, 1 foot square, and 1 foot square with curved approach.

The 2 by 2 feet aperture was formed of four cast-iron plates 0.536, or $\frac{17}{32}$-inch, in thickness, and $6\frac{1}{4}$ inches wide, planed on the sides and edges, and secured together at the corners with bolts and cleats, as shown in Fig. 8. This plate was bolted to the wooden frame inserted in the opening in the vertical bulk-head, and was carefully measured to ascertain its exact dimensions after being put into its place, to provide against any displacement of its parts. The exact dimensions of this aperture, in place, were 2 feet horizontal and 1.99975 feet vertical, making an area of 3.9995 square feet.

The 2 by 1 feet aperture was formed by inserting in the lower half of the above aperture, two plates of the same thickness, planed to the exact dimensions of 6 inches by 2 feet. This was done as shown in Fig. 9. The exact dimensions of this aperture after being put in place, was 2 feet horizontal by 1 foot vertical, or 2 square feet.

The 2 feet by 6 inches aperture was formed by inserting the lower one of the two plates of 2 feet by 6 inches, in the upper half of the 2 by 1 foot aperture. This is shown in Fig. 10. The exact dimensions of this aperture, after being set in the frame for experiment, was 2 feet horizontal by 0.5 foot vertical.

The 2 feet diameter circular aperture was turned in a circular cast-iron plate of 3.08 feet outside diameter planed true on the sides to a thickness of 0.398 or $\frac{13}{32}$-inch, and furnished with bolt holes for attaching it to the wooden frame-work in the vertical bulk-head. When experimented upon the true diameter of the aperture was found to be exactly 2 feet. This plate is shown in Fig. 11.

The 1 foot diameter circular aperture was turned in a similar cast-iron plate of 2.5 feet outside diameter, planed on the sides to a thickness of 0.438 or $\frac{7}{16}$-inch, and furnished with bolt holes for attaching it to the

wooden frame. The true diameter of the aperture, when experimented upon, was proved to be 1.0007 feet. This plate is shown in Fig. 13.

The 6-inch diameter circular aperture was turned in a circular cast-iron plate of 1.5 feet outside diameter, planed true on the sides to a thickness of 0.479, or ½-inch, and was furnished with holes for bolts, as before described for the larger plates. The true diameter of this aperture, when experimented upon, was exactly 0.5 feet. This plate is shown in Fig. 14.

All of these plates were made by the Holyoke Machine Co., under the immediate direction of Mr. Whitten, and were finished as smooth as practicable in the planer and lathe, without filing or polishing.

The 1 foot square aperture was cut in a piece of wrought-iron plate, planed and finished on one side, and was 0.264, or ¼-inch in thickness. This aperture was planed through, across the plate, and was finished by filing. It was true and square and measured, when experimented upon, 1.0000833 feet on each side. It was provided with bolt holes for attaching it to the wooden frame, in the same manner as the other plates, and is shown in Fig. 12.*

The curved approach to the 1 foot plate used in the last experiments tried, where the plate was placed in the bottom of the flume and submerged, was made in a timber framing 2.07 feet square outside, and 6 inches thick. The flare was formed by cutting away the material, back from the face to which the iron plate was bolted, in the shape of a quarter of an ellipse whose semi-diameters were 6 inches and 4 inches, with the largest diameter at right angles to the plane of the plate. This is shown in Fig. 15.

The standard of measurement used for these plates, and for all the measures in the experiments, was a 3 feet scale made by Darling, Brown & Sharps, of Providence, and marked U. S. Standard. The measures for the heads in the flume, and for the lengths of the measuring weir, were carefully transferred to wooden bars of greater length, while the dimensions of the iron plates and the heights on the weir were taken with metallic scales.

A thermometer was suspended in the water of the weir basin, near the middle, to record the temperature.

Method of Conducting the Experiments.—After all was prepared in the flume by inserting the plate in the frame, and carefully measuring the height of the aperture above the weir, the gauges were tested and their scales set at the proper zero, or their readings noted.

* This plate was made by the Colt's Patent Fire Arms & Manufacturing Co., of Hartford, Conn., for Mr. James L. Cowles, of Farmington, to determine the discharge through a square foot aperture.

The aperture was then closed as tightly as possible by means of the cover, and the leakage measured by allowing the water to rise gradually, while the gauges were read every minute. For the small quantity leaking through the bulk-head it was found impracticable to regulate the entrance gate at the head of the lock so as to cause the water to remain at a fixed head in the flume chamber for a sufficient length of time to obtain consecutive observations at the same height; the method was therefore adopted of allowing the water in the flume to rise at a nearly uniform rate and quite slowly, while the gauge readings were taken. When the water had attained the same height as the canal above, the gate at the head of the lock was closed, the cover was removed from the aperture, and the water in the flume was allowed to flow out. When the water was sufficiently out of the flume chamber, the gate was again partially opened in order to admit the quantity of water required to raise the head in the flume a given amount, as say 2 feet, and to maintain it there, at as nearly a uniform height as possible, while a series of observations were taken. The gate was then opened a little more, and an additional head obtained, when another series of experiments was made. This was continued, at intervals, to the top of the flume, or to the greatest head that could be accurately measured.

The experiments were commenced, July 24th, 1874.

The 2 by 2 feet aperture was placed with its frame in the opening cut in the bulk-head, and the hinged gate shut down upon the inside. The chain was attached to the ring-bolt of the cover and connected with the hoisting mechanism. The arrangement of the weir basin was as heretofore described, and the measuring weir was carefully set to a length of 4 feet. The centre of the aperture was compared by levels with the point of the hook gauge and with the crest of the weir. The scales of the hook gauge and the glass tube gauge were set to read the head in the flume upon the centre of the aperture. The centre of the aperture was 1.9 feet above the crest of the weir.

The hook gauge at the weir was so situated that a spirit level could be laid directly upon the top of the bar forming the crest of the weir and the point of the hook. This level was tested before commencing the experiments and very frequently afterwards.

After these arrangements were made, the hinged cover being closed over the aperture, the small gate in the upper lock-gate was a little opened. This allowed the flume to fill up very gradually for the purpose of determining the leakage. As the water rose in the flume the gauges at the flume and weir were read each minute until the full head was

reached. This occupied a little over two hours. The water was allowed to rise gradually and slowly, in order to have as little difference as possible at the same instant of time between the amount of leakage and the quantity of water actually passing over the weir.

After the leakage has been measured for the whole head of 17 feet, an attempt was made to raise the gate over the aperture out of the way to discharge the water in the flume; but, as before stated, after rising a short distance the hinge and chain failed, and the gate fell back partly over the aperture. There was, however, sufficient space to discharge the water, and when it had fallen the whole gate was removed and the experiments upon the aperture commenced. For this purpose the weir was lengthened to 10 feet.

The first series of experiments was with a head of a little over 0.8 feet upon the centre, with the aperture as a weir. The second series of experiments with the same aperture was with a head of about 2 feet, and the third and fourth with heads of about 3 and 3½ feet, respectively. At the latter head the rush of water from the aperture of 4 square feet was so great that it became evident that a farther increase of head would create sufficient undulation at the weir to render the results of the measurement doubtful. As the greatest attainable accuracy was desired in the measurements, the experiments with this large aperture were discontinued, and the next day the water was drawn off from the canal and the changes made in the arrangements of the stop planks that have been already described.

On July 27th, the experiments were resumed with an aperture of 2 feet horizontal, and 1 foot vertical, arranged as already described. The hinged gate was placed over this opening upon the inside of the flume, with the hinge strengthened so as to bear the strain of opening under the pressure of the water. The centre of the new aperture, which was formed by placing the additional plates, 1 foot wide, in the bottom of the 2 by 2 feet opening, was 0.5 feet higher than before, or 2.4 feet above the crest of the weir.

The hook gauge, for measuring the heads on the aperture, was changed to correspond with the new centre, and the scale of the glass gauge was allowed to remain as it was before, during the series of experiments with this aperture. This made the readings of the glass gauge all 0.5 feet too great, as the centre was raised that amount from what it was before

The weir was set to a length of 2 feet to measure the leakage, and the flume was allowed to fill gradually by opening the small gate, as

before, for the purpose of determining its amount at different heads. This was taken up to a height of 17.2 feet, occupying about two hours. When the flume was filled to the level of the canal above, and the head remained nearly or quite stationary, the weir was suddenly lengthened to 10 feet, and as soon as the water in the weir basin had come to a stand, which was in about seven minutes, the head and depth upon the weir, were again observed. This was done to determine the discharge over the weir at very low heads, which were required in measuring the leakage. After concluding the experiments for ascertaining the leakage, the cover over the aperture was raised out of the way, to discharge the water, and the weir was set at 6 feet in length.

The first experiments were made with the aperture as a weir, at heads of 0.08 feet in series No. 5, and about 0.28 feet in series No. 6, measured above the centre of the aperture. In series No. 7, the actual head upon the centre was 0.508 feet or 0.008 above the top of the aperture; the water, however, did not touch the top edge, but the aperture was still flowing as a weir.

In the series of experiments, Nos. 8, 9 and 10, the head upon the aperture was run up to 4.7 feet, with the weir remaining at the length of 6 feet. At the end of the series No. 10, the weir was suddenly lengthened to 10 feet, and a series of observations taken after the weir basin had again come to an equilibrium, which took about four minutes.

In the series from No. 11 to No. 16, inclusive, with the same aperture, the weir remained 10 feet in length, and the head was raised by intervals to 11.8 feet. This was the greatest head that could be used with this aperture without creating undulations over the weir, which might affect the accuracy of the measurement.

Experiments were next tried with an aperture of 6 inches vertical by 2 feet horizontal. This was formed with the same plates by placing the bottom strip of 6 inches wide in the top of the 2 by 1 foot aperture, leaving the bottom edge at the same height as before. This made the centre of the new aperture 2.15 feet above the crest of the weir.

The weir was at first set to the length of 5 feet. The hook gauge at the flume was set to the centre of the aperture and the scale of the glass gauge left as before, reading therefore 0.25 feet too much for the new centre, and requiring this amount to be subtracted from the observed heights.

Seven series of observations, from No. 17 to No. 22 B inclusive, were then taken, at heads varying from 1.4 to 11.8 feet, when the weir was changed to 10 feet in length, and as soon as the level of the water in

the weir basin came to rest, which occupied about 4 minutes, a series of measurements was taken at the same head last observed. Three series of experiments were then made, No. 23 A, No. 23 B and No. 24, running the head up to 17.2 feet, which was then the level of the upper canal.

At the conclusion of the above experiments, the rectangular plates were removed, and that with the 2 feet circular aperture put on in the same place and upon the same framing. The gauges were examined and changed to conform to the centre of the new aperture, which was found by measurement to be 1.97 feet above the crest of the weir. The hook gauge was lowered 0.18 feet, and the glass tube gauge raised 0.07 feet to conform to the new centre. The weir remained with a 10 feet opening.

Seven series of observations, No. 25 to No. 31, inclusive, were taken with heads from 1.8 to 9.7 feet.

The foregoing experiments continued during July 27th, 28th and 29th, and were then temporarily discontinued in order to use the flume during the next two weeks for testing turbines. The experiments were not resumed until August 12th, when the flume was again prepared for continuing them.

The stop planks put into the weir basin having been partially removed during the testing of the turbines, a somewhat different arrangement was adopted during the succeeding experiments. These changes have been previously stated in the description of the weir basin.

The plate with the 1 foot square aperture was placed in the flume, upon a frame of smaller dimensions than the one used with the larger openings. For the purpose of closing the apertures upon the inside, while testing for leakage, the loose cover was used instead of the hinged cover before described. This cover was attached to the hook of the chain from the hoisting apparatus, for removing it when the flume was full. It was placed over the aperture, and the gauges were all tested and adjusted to the height of the centre of the aperture, which was 1.556 feet above the crest of the weir. The weir was set to a length of 2 feet, and the hook gauge was adjusted and compared with its height.

The flume was then allowed to fill up slowly and gradually, as before, to determine the amount of leakage, the time occupied in this operation being one hour and twenty minutes.

When the full head of 17.7 feet was reached, the weir was lengthened to 4 feet, and a series of observations with the same head taken, as soon as the level of the surface in the weir basin had become stationary, which was nineteen minutes after the weir was changed. The cover upon the

inside of the aperture was then lifted off and the water contained in the flume allowed to escape.

A series of observations, No. 32, was first taken with the aperture as a weir, at a head of 0.516 feet above the centre of the aperture. The depression of the surface was such that the water did not touch the upper edge of the orifice.

Several series of observations were then taken—Nos. 33 to 38, inclusive—with heads from 1.5 to 9.85 feet, the measuring weir remaining at 4 feet. It was then lengthened to 9.013 feet, and, as soon as the level of the weir basin came to rest, a series of gauge readings was taken at about the same head. The time occupied in lowering the level in the weir basin was about eight minutes. The weir was then lengthened to 10 feet, and the experiments continued—series Nos. 39 to 42, inclusive—until the full head of 17.56 feet was reached.

The water was then allowed to run out and the 1 foot circular aperture substituted for the 1 foot square plate. The centre was set at the same height as before, 1.556 feet above the crest of the weir, so that the gauges did not require alteration. The weir was set at 4 feet in length. Four series of experiments, Nos. 43 to 46, inclusive, with heads varying from 1.15 to 7 feet, were then made with the weir at this length. It was then lengthened to 10 feet, and when the surface of the weir basin had come to rest, which was in about five minutes, another series of observations of the weir heights was taken. Five series of experiments, Nos. 47 to 51, inclusive, were then made at heads varying from 10.9 to 17.74 feet with the weir at this last length.

The experiments were again suspended, from August 13th to 18th, on account of the flume being needed for testing turbines, and were resumed on the latter date with the same arrangement of apparatus, and of stops and floats in the weir basin, as before.

The 1 foot circular plate was removed and the one with aperture of 6 inch diameter put in its place, with the height of its centre 1.806 feet above the crest of the weir. The hook guage at the flume was set to read from this point, and the scale of the glass gauge remained as before.

A hole was cut in the planking which covered the opening in the bottom of the flume, and the circular aperture of 1 foot diameter was placed over it by screwing the plate before described down to the planking, the timber being cut away sufficiently to give a free delivery from the aperture. The top of this plate was 0.457 feet above the crest of the weir.

At one side, in the extreme bottom of the flume, an annular cast-iron frame was set in the planking and fitted with a cover, resting upon it in a

groove, nearly or quite water-tight. This was for the purpose of drawing off the water in the bottom of the flume, below the aperture in the side, which became necessary in arranging the submerged apertures placed in the bottom. Previous to this, when it became necessary to let the water out of the bottom, one of the planks was removed, which covered the opening over which turbines were placed in testing. The cover of this discharge opening had a loop into which a rod was hooked which extended upward through the flooring and was raised, when desired, by the hoisting apparatus.

All these arrangements having been completed, the weir was set at 2 feet length, and a series of experiments made to determine the leakage. This was taken up to a head of 17.3 feet upon the centre of the 6-inch aperture. The gauges were then examined and the scale of the glass gauge was made to conform to the centre of the aperture.

Two series of observations, Nos. 52 and 53, were then taken, at about 2 and 4 feet heads, with the 6-inch aperture, the other being closed. The weir was lengthend to 4 feet, and a series of observations, No. 54, taken with a head of a little over 6 feet.

As it was found somewhat difficult to obtain the required heads in the flume, and to maintain them for any length of time with this small aperture, on account of the large volume of the chambers above and below the bulk-head, in comparison with the size of the aperture and weir, the plan of adjusting the opening of the gates to admit a volume sufficient to equal the flow at a given head, so that the level in the flume would remain nearly or quite constant, was changed to that adopted while taking the leakage, of letting the head in the flume slowly rise while the readings of the gauges were taken. In this manner the head was run up to 17.3 feet, the level of the upper canal.

The water was then drawn off, and the 6-inch aperture closed, so as to experiment with the apertures in the bottom of the flume. The water was allowed to rise in the flume until there was no vortex over the aperture. The circular motion ceased when the height above the aperture was about 2.6 feet. Four series of observations were then taken, Nos. 55 to 58, with heads up to 8.2 feet and the weir at 4 feet in length. This was then lengthened to 10 feet, and another series taken with the water at the same height in the flume. The difference of height on the measuring weir, however, made the head on the aperture somewhat greater, or about 8.8 feet. The orifice being submerged, the head was measured by the difference of level between the surface of the water in the flume and that of the weir basin.

Four more series of observations were then taken, Nos. 59 to 62, running the head up to 18.7 feet. In these experiments with the 1 foot round aperture, the level of the water in the weir basin was always above the top edge of the aperture, so that it was at all times submerged; the volume of water discharged passed down into the space under the floor of the flume before described, and thence out to the weir basin. The bottom of the planking of the flume was about on the level of the crest of the weir, and there was a space of about 6.5 feet under it to the bottom of the basin.

At the conclusion of this series of experiments, the plate containing the 1-foot round aperture was taken off, and the 1-foot square plate screwed down in its place. This plate was $\frac{3}{16}$ inch thinner than the round plate, so that its upper surface was that much lower, or 0.441 feet above the weir.

The weir remained at 10 feet in length, and seven series of observations were made, Nos. 63 to 69, inclusive, at heads from 2.2 to 18.5 feet. With the lowest head there was no vortex formed, the water being raised. until the vortex disappeared, before the observations were commenced. At the lowest head observed, the level of the weir basin was only 0.37 feet above the crest of the weir, while the level of the top surface of the aperture was 0.441 feet above the same point; so that it was theoretically 0.07 feet above the point at which it would be submerged. As, however, this space was enclosed within the thickness of the plank flooring of the flume, and must have been entirely filled with water, it probably made no material difference in the discharge. The head in all the experiments was measured from the surface of the water in the flume to the surface of the water in the weir basin.

At the conclusion of the foregoing experiments upon sharp edged apertures, it was desired to know the discharge through a certain shaped mouth-piece placed inside of the aperture. This consisted of an elliptical curved approach to the iron plate, cut in a square wooden frame. This frame was fitted to the plate and was fastened down over it, so that the edges of the apertures exactly coincided. This elliptical approach has been previously described and is shown in Fig. 15. Five series of experiments, Nos. 70 to 74, were made with heads from 3 to 18.2 feet. The lowest head was taken as soon as the vortex ceased over the opening.

These last experiments were made on the afternoon of August 19th, and concluded the series, as the flume was again required for other purposes.

Velocity of Approach.—In the foregoing experiments the velocity of approach to the measuring weir was rarely sufficient to affect the volume of discharge. It has been, however, in all cases taken into account in the computations, and the proper correction made. These corrections are generally so small that the difference in the theories of the more distinguished writers upon hydraulics regarding the effect of the velocity upon the discharge, would make but slight differences in the results. Experiments upon this subject appear to be meagre, unsatisfactory and unsuited to the present requirements, on account of the very small sections of the channels in which they have been tried. Different authors have been led by them to different conclusions, which would greatly affect the quantity discharged over a weir, if the velocity of the approaching current should be considerable.

Boileau and some other writers assume that the whole living force of the approaching current, in the entire section of the stream, is expended in increasing the discharge over the weir. Others, and among them Mr. Francis, assume that the living force of the current in a section equal to the area of the orifice only, is expended in increasing the discharge. As Mr. Francis observes, neither of these assumptions is probably strictly correct.

Let us for a moment observe what would be the probable conditions under which water would approach a weir through a canal of larger area, the weir being opened through a square bulkhead in the usual manner. In the channel, the water would flow with a velocity greatest in the middle at, or a little below, the surface, and diminishing towards the sides and bottom of the canal. As this current flows towards the weir opening, it converges, becomes more rapid and passes over the weir, leaving more or less dead water without motion, in the angles formed by the bulkhead with the bottom and sides of the canal.

Now, it is evident from the elementary principles of mechanics that the whole living force of the stream is expended *somewhere*. The important question is, when and how does it affect the discharge? From a somewhat careful study of the motion of the filaments of water as they approach and are discharged at the weir, the writer is of opinion that the greater part of the current at the bottom and sides of the channel, outside of the dimensions of the weir opening, exerts its force in maintaining a head near the weir due to its velocity by means of its reaction against the bulkhead or the dead water in the angles. This head is measured by the gauges which show the depth upon the weir, and is the exact equivalent of an equal head of still water, so far as relates to the discharge.

The central upper portion of the stream, opposite the weir opening, being unobstructed in its motion by the bulkhead, or by any material friction from the sides, imparts its living force directly to the volume passing over the weir and increases its discharge without increasing the observed head as measured by the gauge. In determining the velocity of approach which acts directly upon the discharge, we should therefore take that part of the stream which is opposite to the weir opening, and not the mean velocity of the whole stream, as is generally done.

It is not thought that the boundary of the weir opening is the exact limit outside of which the living force of the stream only increases the measured head, or within which it only increases the velocity of discharge over the weir, but that it is the nearest practicable approximation to the operation of the existing forces and their effects upon the discharge.

As this velocity of approach is combined with the velocity with which the water would flow over the weir from a still pond, and as it may be considered uniform for the length and depth of the weir aperture, it is customary to ascertain what head would produce this velocity and add it to the actual measured head upon the weir, to find the effective head under which the flow takes place. This effective head does not increase the actual depth upon their weir, but adds to the velocity of discharge without increasing the discharging area. The theoretical velocity without contraction is easily obtained. The velocity of water flowing over a weir is proportionate at the several depths to the ordinates of a parabola which has its vertex at the height of the head and a parameter of $2\,g$. The total discharge is, therefore, represented by the area of the segment included between the vertex and the ordinate representing the bottom velocity at the crest of the weir. In the case before us, we have the theoretical discharge equal to the parabolic segment of which the height to the vertex is $H + h$, less the parabolic segment whose height to the vertex is h; H and h being respectively the measured height on the weir and the head due to the velocity of approach. This gives the exact theoretical discharge and may be expressed by the formula, given by Mr. Francis;* $\frac{2}{3}\sqrt{2g}\left[(H+h)^{\frac{3}{2}} - h^{\frac{3}{2}}\right] l$.

In obtaining the true discharge, he applies to this formula his coefficient obtained for weirs, which, although not proven for this formula, is evidently near enough for all practical purposes of application and, agrees well with such experiments as have been tried. Neville, in his "Hydraulic Tables and Formulæ,"† makes the flow over a weir under

* Lowell Hydraulic Experiments. J. B. Francis, p. 117. † Page 101.

these circumstances exactly equal to the discharge through a rectangular aperture of the length of the weir with a height H and a head of h above the upper edge. This appears to be substantially correct, although at first sight it would seem that the top contraction would diminish the discharge. The bottom contraction is, however, proportionately less. In reality, instead of there being any top contraction, there is an increased swell or change of theoretical curve in the other direction, nearly or quite equal to the contraction due to the discharge under the head h.

In the present experiments, the effect of the velocity of approach upon the discharge over the measuring weir has been computed by taking the total actual discharge at a head of $H + h$, and deducting from it the actual discharge, with a head h upon the same weir. This is believed to be the most accurate method of obtaining the true discharge, and has also the advantage that the quantities can be taken directly from prepared tables of discharge. Mr. Francis's method is laborious in all cases, and is based upon the same general principle.

Mr. Francis takes the difference of the theoretical discharges at heads of $H + h$ and h, and applies his coefficient to the result; or more exactly he finds a height H^1, which will produce this difference of discharge with any uniform coefficient, and then applies his coefficient to the theoretical discharge at a head equal to H^1. This assumes that the bottom and end contraction is the same in amount for the discharge at a height H^1 as for the discharge at a height H under a head of $H + h$. At high velocities this involves a small error which is, however, more theoretical than practically important. The corrections for velocity of approach are applied to the present experiments in all cases where they affect the result by so much as 0.01 cubic feet per minute.

Leakage.—Before commencing the experiments, and as often as any change was made which would affect the amount of leakage from the upper side of the bulkhead in which the apertures were placed, to the lower sidě from which the water flowed over the measuring weir, the leakage was taken by noting the depth on the weir, for all heights in the flume that were to be used in the experiments upon the apertures.

The water was admitted to the lock above the bulkhead, and allowed gradually and slowly to rise until it attained the full head. The gauges were carefully read at intervals of one minute, while the head slowly increased. The head was allowed to rise very slowly at first, when greater accuracy was more important on account of the leakage being a larger percentage of the total discharge through the apertures, and

afterwards more rapidly as the head became greater. The whole operation usually occupied about two hours.

There was probably some leakage through the weir bulkhead, although none was apparent. Whatever this leakage may have been, it was small in amount and was of no material consequence in the experiments, as the amount measured over the weir gave the difference between this and the quantity leaking through the bulkhead above, which was, even at the lowest heads, always in excess; so that there was always water running over the weir. Whatever leakage there may have been in any manner from the basin above the weir, was thus eliminated from the results of the experiments.

The nature of the leakage through the bulkhead in which the apertures for the experiments were placed, was as follows:—below the surface of the water in the basin between the bulkhead and the weir, there was evidently some leakage; the water would run over the crest of the weir when there was an appreciable difference of head. As the head increased there was little or no visible leakage above the level of the weir basin until the opening in the bulkhead was reached in which the plates were set for the experiment. The joints were not perfectly water-tight, although every care was taken, by packing or otherwise, to have them as nearly so as possible, and some small jets would find their way through. There was also a probable leakage through the hole in the bottom of the flume above the bulkhead, which was ordinarily left open while work was going on in placing and arranging the apertures to get rid of the water remaining below the aperture, and which was spiked down the last thing before leaving the flume and opening the gate to admit the water from the canal. There was, probably, a small leakage under the covering plank next to the floor of the flume. This was 0.2 feet above the crest of the weir. From this place up to the opening made for the frame which held the plates experimented upon, there was little or no additional leakage; around the plate, as before stated, some water escaped; above this there were occasional leaks between the edges of the bulkhead and the masonry of the sides of the lock, which became more apparent as the head was increased in the flume.

In the first two experiments for leakage, there was a remarkable change in the rate of increase, commencing at about 10 feet head; apparently indicating an unobserved interior leak beween the flume and the weir basin. In the first series of observations taken, the rubber tube leading from the flume to the hook gauge was accidentally left open, which would account for some extra leakage in that experiment; it was

not observed until a number of gauge readings had been taken, so that it was left open during the remainder of the trial for leakage and in the subsequent experiments for which this leakage was determined ; being a constant quantity for the same head, it did not affect the results ; it had only the effect of making the leakage somewhat larger than it otherwise would have been.

The cause of the leakage increasing so much more rapidly at high heads in the first two series of experiments than in the subsequent ones, has not been determined. There must have been some openings not seen above the surface of the water, caused, perhaps, by the springing of the plank bottom of the flume and opening the joints, which did not occur in the subsequent trials. There was an interval of two weeks between the experiments, during which the flume was used for the testing of turbines and a consequent taking out and replacing the bottom flooring, which may have, and probably did, affect the amount of leakage. The actual flow over the weir was in all cases carefully measured, at all heads, before commencing the experiments upon the apertures, after each change requiring it ; so that no ambiguity existed regarding the actual volume of leakage over the weir with the aperture closed. When, however, the aperture was opened to measure its discharge, the depth of water upon the weir was increased for the same head in the flume above ; the leakage, therefore, was not all acting under the same head as before ; the water pouring into the basin from the large apertures with which the experiments were tried, filled it to a higher level and created a back water, so to speak, upon a certain portion of the leakage previously measured.

Thus, above the new level in the weir basin, the amount leaking through the bulkhead would remain the same for the same head. Between the new level and the previous one the amount leaking through would be diminished by the mean difference of head ; below the line of the water in the weir basin, at the time the leakage was measured, the amount would be diminished by the quantity due to the difference of head between the two levels in the basin : that is, there would be back water upon the leaking apertures below the surface, to this amount.

Although all of these quantities are small, they have been taken into account in the computation. Fortunately there was no perceptible leak between the level of the weir, or low water in the weir basin, and the highest water during the experiments. This eliminated the consideration of the leakage between those points. The leakage, therefore, became divided into two parts or elements, one of which was that proceeding from openings in the bulkhead above the bottom of the aperture upon which

the experiments were tried, and which was independent of the level of the weir basin, and the other proceeding from whatever openings might have existed below the surface of the water in the weir basin at the time the leakage was measured. This part of the leakage was dependent upon the difference of the level in the flume and weir basin, which was greater for the same height in the flume when the leakage was measured, than afterwards, during the experiments, when a greater volume of water was discharged over the weir.

At first the problem of determining what proportion of the leakage was affected by the reduced head, seemed to present some difficulty, as the total amount only had been measured, and that only from a point above the centre of the aperture upon which the experiments were to be tried. Although it was evident that the amount of difference was small, yet it was desired to obtain every element in the computation of discharge with the greatest exactness, and to determine the amount that the total leakage would be reduced by the lessened head on that part of it which was below the surface of the water. It was finally obtained in the following manner:—the leakage discharges, as measured, were plotted as ordinates upon section paper to a large scale, which would show 0.01 cubic feet per minute. This gave the general direction of a line of which another point was 0 discharge at the height of the weir, or at the head of 0; the heights in the flume, from the weir up, being plotted as abscissæ. All the leakage below the lower level of the weir basin was assumed to pass through an opening below the surface of the water, which would give the proper discharge at the height of the lower edge of the experimental aperture. As any further leakage came in above this point, the approximate leakage at the bottom of the aperture was assumed to be the quantity passsing through below the water at this head, and the curve of theoretical discharges due to the same opening was carried up to the higher heads at which experiments were made.

If, then, we call L the total leakage measured for the height of water in the flume in any experiment, h the difference of head between the level in the weir basin and the flume, and d the difference between the level in the weir basin and the previous height when the total leakage was measured, we shall have the true leakage equal to L, less the difference of discharge through the submerged apertures at the heads $h + d$ and h.

In this manner the leakage was found for all the experiments. The exact values of the submerged discharges were not material, as their differences only entered into the computation, and under the small changes

of level in the weir basin for which the differences of discharge were taken, the size of the equivalent submerged orifice could be materially varied without sensibly affecting the results.

CORRECTION FOR DIFFERENCE OF LEVEL IN THE WEIR BASIN.—There was another important correction to be made in the volume measured over the weir, to determine the exact discharge from the aperture in the bulkhead. In the experiments, it was endeavored to maintain the water in the flume at as nearly as possible a uniform height during a series of observations, by the adjustment of the gates leading into the lock from the canal. It was found, however, that when the level in the flume attained nearly its position of equilibrium, so that the volume of water discharged at the orifice would be nearly equal to that entering, it rose or fell very slowly, and that the changes of level in the canal above would modify its height in such a manner that it became impossible, by any amount of waiting, to obtain an exact uniformity of level in the flume and weir basin for several consecutive minutes.

The fluctuations in the level of the weir basin followed those of the water in the flume. If the head in the flume was rising, an increasing volume would be constantly pouring into the weir basin, and necessarily raised its level before the weir would discharge it. If the head in the flume were decreasing, the lessened volume entering the weir basin would cause it to lower its level until the weir discharged the reduced quantity. If the change in the level in the flume were going on gradually, or was continually fluctuating, the changes in the weir basin would likewise be continually taking place, a little in retard of the changes in the flume.

These changes did not follow with equal rapidity in rising and falling. For instance, suppose the flume level to rise quickly a certain distance and remain stationary, the increased volume of discharge would very soon fill the weir basin so that the weir would discharge the additional quantity. If, however, we suppose the level in the flume to drop quickly the same distance and remain, then the water in the weir basin would not so quickly adapt itself to the new discharge. The extra height would gradually run off, but the fall through the last part of the distance would be extremely slow.

For this reason the observations were taken, in nearly all cases, while the water was as nearly stationary as possible, but were commenced while it was still rising, with an endeavor to seize the opportunity of taking some readings while the height would be stationary in the flume and weir basin. This, however, seldom occurred, and it is doubtful if the results of a single observation, or even a series of observations, taken when the

water was stationary, would give any greater degree of accuracy than a succession of observations taken when the water was gradually rising and treated as will be described. With the leakages, and observations upon the small circular aperture of 6 inches diameter at the higher heads, it was found impracticable to regulate the entrance gates so as to obtain such an equilibrium of discharge between the gates and the orifices as to maintain a given head. The nicest practicable adjustment in the opening of the gates caused so great and uncertain a difference of head in the flume before coming to a stand, and occupied so much time in doing so, that these observations were taken with a constantly rising head at a slow and nearly uniform rate. The reading of the gauges were taken at one minute intervals and at the same time.

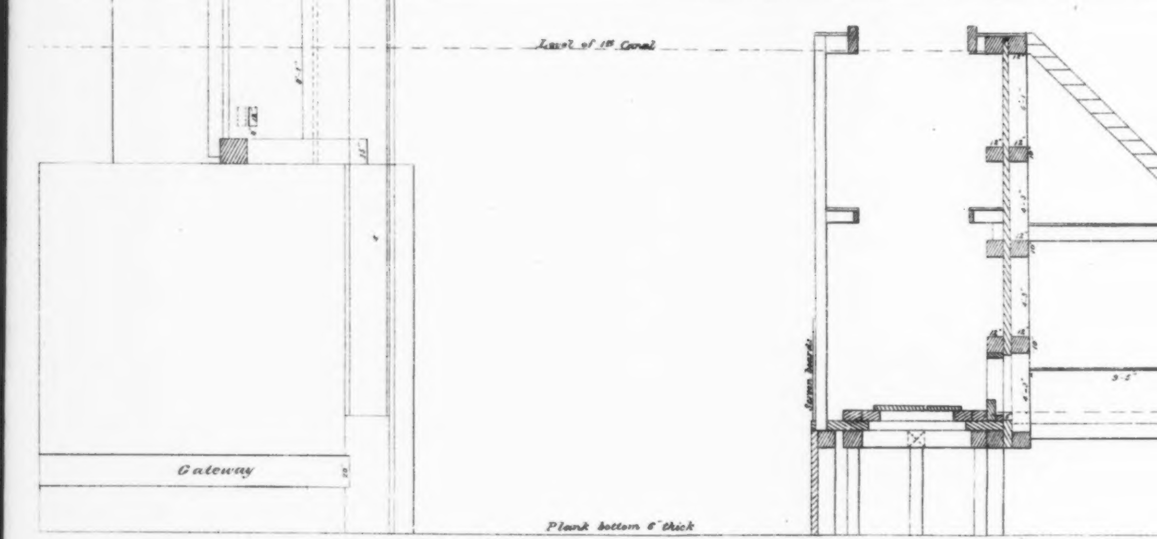
Although the changes of head in the flume and weir basin were very slight and gradual, it will be observed that the quantity of discharge through the aperture would always precede the same amount passing over the weir by a certain interval, owing to the time occupied in raising or lowering the level of the weir basin to the proper height to discharge this quantity. A correction was therefore required which should make the measured discharge equal to that flowing from the aperture at any given instant of time.

The constantly varying head in the flume, and the constantly varying discharge over the weir, together with the fact that the times of equal discharges were not synchronous, appeared at first to offer an obstacle to deducing more accurate results from the observations than could be obtained by mechanical methods of getting the head upon the aperture and the level of the weir basin as nearly in equilibrium as possible, and measuring the resulting head upon the weir. The practical difficulties of maintaining an exact head were, however, such that in most cases single observations would have to be depended upon in place of the means of several successive experiments.

After investigating this subject mathematically in various ways, in order to ascertain the laws upon which the relative variations in the quantity of discharge by the apertures used in the several experiments and that of the measuring weir depended, it was found that the most accurate and determinate method of obtaining the exact discharge at a given time was likewise one of the simplest and most readily comprehended.

It is evident that at any instant of time during which an aperture is discharging into the weir basin, and therefore at a known head in the flume, although this may be constantly varying, the differential of the volume discharged will be equal to the differential of the quantity passing

F


Level of 1st Canal
Screen boards
9-5"
Gateway
Plank bottom 6" thick

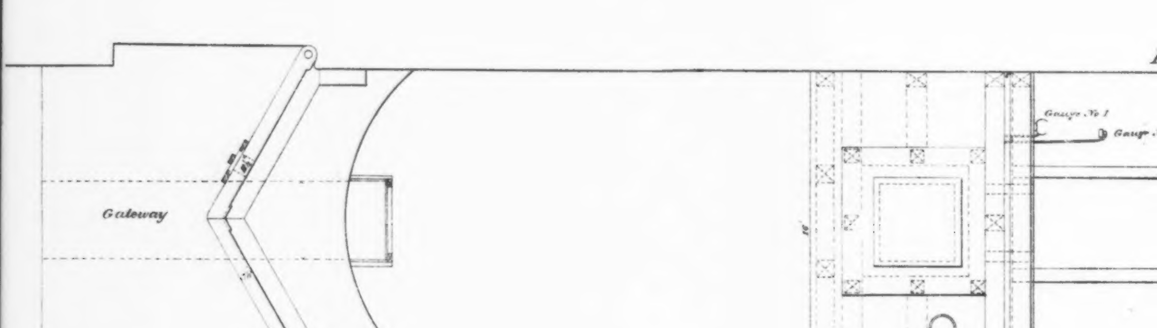

Gauge No 1
Gateway

Plate I.

Fig. 1.

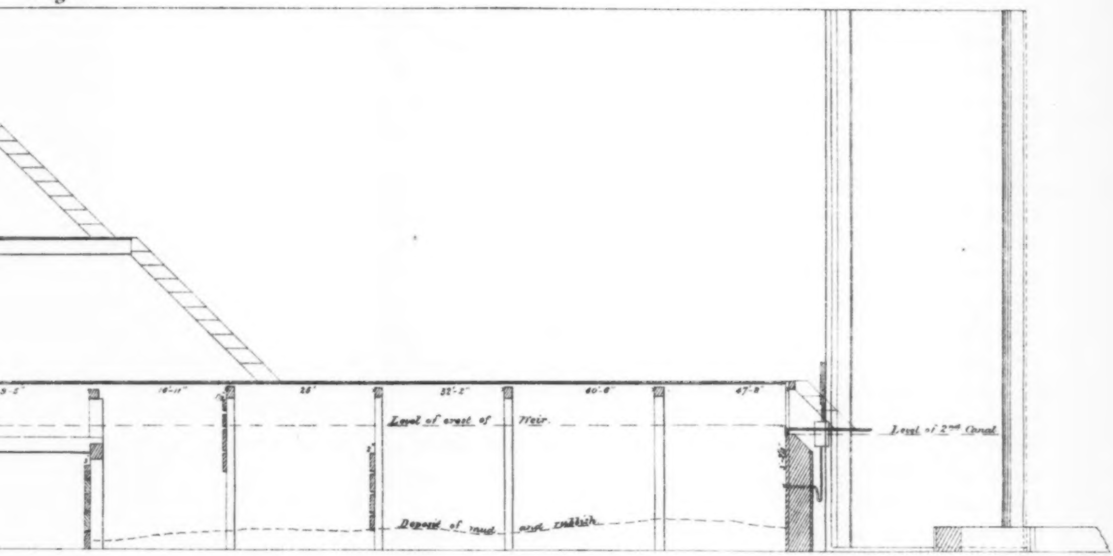


Fig. 2.

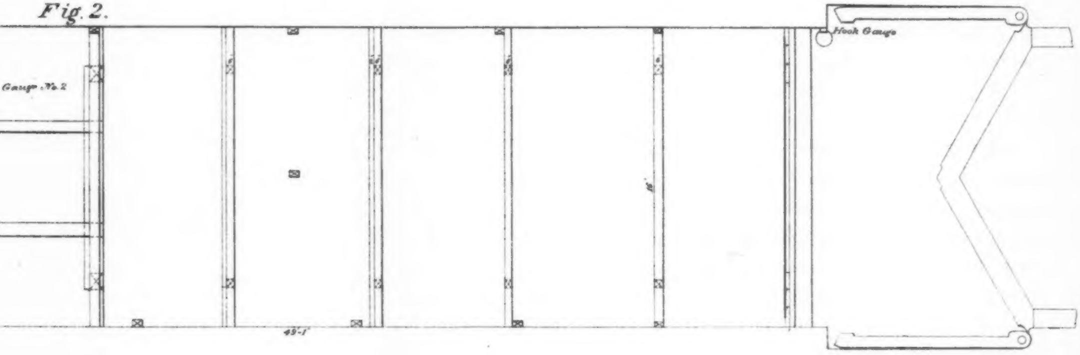


Fig 11.

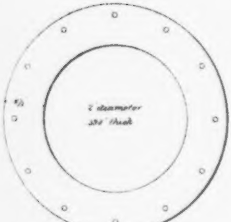


Fig 8

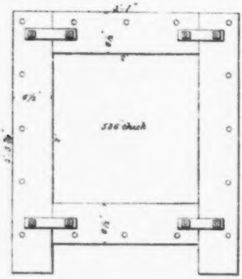


Fig. 16

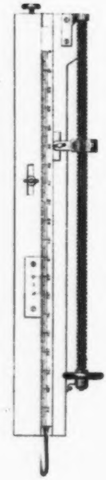


Fig 13.

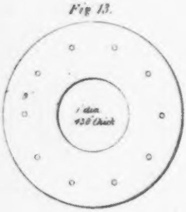


Fig. 9.

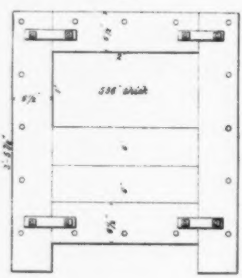


Fig 14

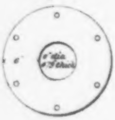


Fig 10.

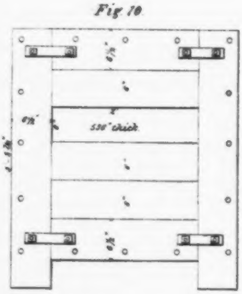


Fig 12

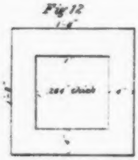


Fig 15

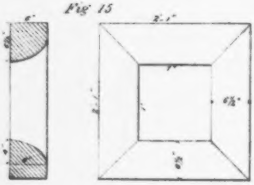


Plate II.

Fig 3

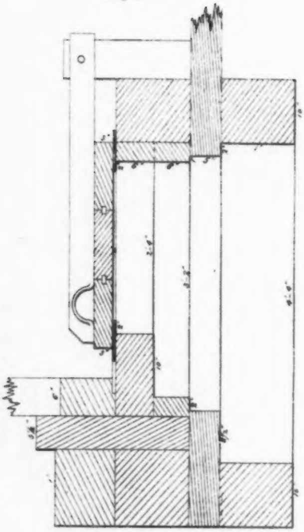


Fig. 4

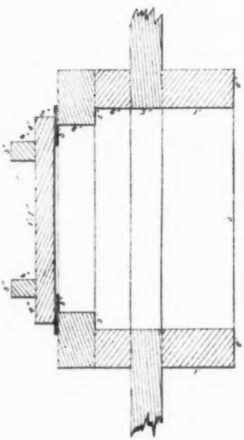


Fig 7

Fig 5

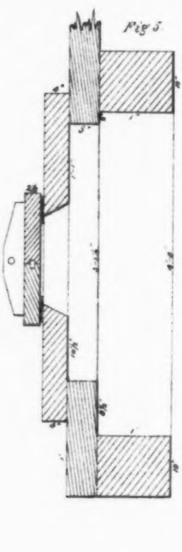
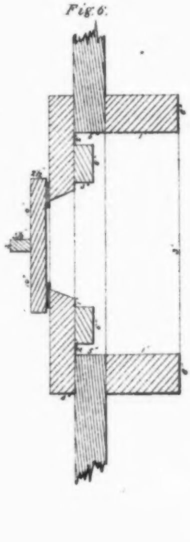


Fig. 6.



over the weir at the same instant, plus or minus the differential of the quantity of water in the weir basin according as it is rising or falling. That is, for any indefinitely small interval of time, the exact rate of discharge from the aperture will be equal to the rate of discharge from the weir, plus or minus the rate by which the quantity in the weir basin has changed, according as the level has risen or fallen.

The rate of discharge over the measuring weir for any instant, we have directly by noting the depth on the weir, and we find by an examination of the several series of observations taken, that the depths on the weir in successive minutes, or the changes of level in the weir basin, vary nearly in an arithmetical progression, so that the changes are nearly or quite alike for two successive minutes. The mean rate of rise or fall for a minute before and a minute after the time will therefore be the rate at the required instant.

In order to obtain the number of feet per second due to a rise or fall of the water in the weir basin, its area was carefully measured and all obstructions, such as vertical posts, stop-planks, etc., were also measured and deducted from the surface area. This area, multiplied by the rise or fall per minute in feet, gave the cubic feet per minute due to its rate of variation. We can therefore determine by this method the exact rate of discharge from the aperture at any given time.

The same principle was applied to the means of the series of observations taken at any particular height, by which very much greater accuracy was obtained than would be possible with any single observation. These series generally differed by but very small quantities, both in the head in the flume and the depth on the weir. The means of these were corrected by the means of the differences of level in the weir basin at the commencement and end of the series.

Methods of Computing the Results.—In combining and computing the results of the experiments, in order to obtain the coefficients of discharge for the several apertures, at the different heads, the series for each aperture taken at practically the same head were carefully examined, and sometimes a few of the observations, generally those taken at the commencement of the series, before the water in the flume and weir basin had come nearly to a stand, were rejected from the computation. The remaining observations constituted a series of nearly the same height on the measuring weir, and nearly the same head on the aperture. The fluctuations were very small compared to the total quantities discharged, and any one of the observations would give a great degree of exactness were the discharge computed from it alone. Many successive

observations were often alike in each series, and the discharge remained practically constant during the time they continued, generally three or four minutes near the end of the series.

When the readings of the gauges were constant for a number of minutes, they gave the volumes of discharge with as great accuracy as the readings were taken; but if there was a small difference in the successive minutes, a correction for the rate of variation in the weir basin became requisite.

As the depth of water upon the measuring weir could only be accurately read to thousandths of a foot, there was reason to believe that the means of a series of ten or twelve observations, when corrected for the slight variations before named, would give more accurate results than the uniform readings even for several successive minutes, owing to the elimination of errors of observation and scale readings that might occur with the uniform height.

The series of observations were therefore treated in several different ways in computing results. In nearly all cases the mean of the whole series was taken, by averaging the heights of water in the flume and the depth on the measuring weir and correcting the resulting discharge by the mean amount of rise and fall in the weir basin. The means of the best few nearly uniform observations were also taken, in different parts of the series, and corrected, if necessary, in the same manner. The results of the best uniform observations not needing correction were also computed.

In some of the series but one of these methods was adopted, as for instance, when the water rose gradually from the commencement to the end of the observations. In this case nothing would be gained by taking less than the whole number. In the greater part, however, two or three methods of combining the observations have been used, neither of which could with certainty be said to be the best, although preference was given them, before computing, in the order in which they are placed in the tables of results.

The general method of computing the volume of discharge was as follows:—the mean was taken of a certain number of observations, noting the means of the heads on the aperture and the depths on the weir to the nearest ten-thousandth of a foot. The approximate discharge over the weir was then taken from a table prepared from Francis's weir formula and noted. The difference was taken of the depths on the weir at the commencement and end of the observations embraced in taking the means, and a correction made for the mean rise or fall of the weir basin by

adding or subtracting the proper quantity to the volume of discharge, as previously described when discussing this correction. The difference of depth on the weir to produce this difference of discharge was also computed.

The correction of discharge for velocity of approach was next computed in the manner heretofore described. This correction was of immaterial amount in most of the discharges, but was made for all, and included whenever it amounted to 0.01 cubic feet per minute. The velocity was deduced from the approximate discharge and the area of the basin a short distance back from the weir. The head due to this velocity was then taken from a table, and the corrected discharge computed as described when discussing this correction. The leakage was then computed. The difference of level in the weir basin when the experiments for leakage and discharge were made under the same head was noted. The leakage for the whole head was corrected by the amount due to this difference, and the result deducted from the computed discharge over the weir. This gave the true volume flowing from the aperture under the given mean head in the flume.

It then remained to compute the coefficient of discharge. The first question that presented itself for consideration was, whether this coefficient should express the ratio between the actual volume of discharge and the true theoretical discharge, or the ratio between the actual volume of discharge and the discharge computed from assuming the head on the centre of the aperture to be the mean head under which the discharge takes place.

Although the former has been advocated as being theoretically the most precise, and such coefficients have been given by some authors, it is believed that the latter is the most usual and by far the most convenient for practical use. There appears to be no necessity for knowing the exact theoretical discharge, and its computation is laborious and difficult even for rectangular apertures. For circular and other forms, it is believed that the exact theoretical discharge is rarely computed. For the above reasons, and also that the best coefficients known give the same ratio, the latter has been computed for these experiments.

The assumed theoretical discharge is therefore $\sqrt{2\,g\,h} \times A$, cubic feet per second, or $60\sqrt{2\,g\,h} \times A$, cubic feet per minute; g, being the force of gravity at the place; h, the head on the centre of the aperture; and A, the area of the aperture in feet.

The coefficient is, $\dfrac{D}{60\sqrt{2\,g}\sqrt{h} \times A,}$; D being the actual discharge.

The force of gravity, g, at the place where the experiments were made was computed as follows:—the latitude of the place, as determined from the maps of the Massachusetts surveys, is 43°-12′-43″, and the height of the orifices was about 81 feet above the mean level of the sea, as determined by levels taken under the direction of the writer from Saybrook to Holyoke, a distance of about 80 miles.

The value of g is computed from La Place's formulas:—

$$g = g' \,(1 - m \cos 2\, L) \left(1 - \frac{2\, e}{r}\right)$$

$$r = R\,(1 + n \cos 2\, L.)$$

m and n being constant factors; L, the latitude of the place; e, the elevation in feet above sea level; r, radius of the earth at the place; g', the force of gravity, and R, the radius of the earth, at latitude 45°.

Using constants computed from Bessel's determinations, on account of their being more recent and accurate than those of La Place, we have,

$$r = 20888625 \,(1 + 0.0016742 \cos 2\, L) = 20892025 \text{ feet.}$$

$$g = 32.1695\,(1 - 0.0026257 \cos 2\, L)\left(1 - \frac{2 \times 81}{20892025}\right) = 32.16107 \text{ ft.}$$

$\sqrt{2\,g} = 8.02011.$ Log = .9041802

Log 60 = 1.7781513

Log $60 \sqrt{2\,g} = 2.6823315$

From the expression given for the value of the coefficient, it will be seen that the sum of the arithmetical complements of this logarithm, the logarithm of the square root of the head, and the logarithm of the area of the aperture, added to the logarithm of the corrected discharge, less 30, gives the logarithm of the coefficient. In this manner all the coefficients given in the tables have been computed.

In computing the quantities flowing over the weir in the experiments with the apertures, Francis's weir formula was used. There were no depths upon the weir less than 0.28 feet, and but very few less than 0.5 feet. The proportions of length and depth were also such as would bring them much within the limits of the proportions indicated in his experiments. The conditions as to the character of the weir and the approach to it, were made as nearly as possible similar to those of the weir he describes, so as to apply the results of his experiments to similar circumstances.

With the leakages, however, the measurements over the weir were of smaller quantities than any contemplated in the use of Francis's formula, or for which it was designed by him to be used; some of the heights

on the weir were as low as 0.05 feet. Below 0.09 feet also, the contraction on the crest of the weir was imperfect, owing to its thickness. For these low heads, therefore, a different formula became necessary.

A comparison was made between Francis's formula and the results of experiments by Poncelet and Lebros, and other observers, taken with smaller heads, to ascertain their differences; and for the purpose of obtaining the actual discharge of the weir at heads of from 0.05 to 0.09 feet, a comparison was made between long and short weirs with the same volume of discharge, the shorter weir running clear from the sharp inside edge of the crest, and the longer weir being below 0.09 feet in depth of water, so that the weir followed the top surface of the crest.

The discharge was in all cases greater with the lower heads, commencing at 0.25 feet in depth on the weir, and increasing to 6 per cent. more than the amount given by Francis's formula, when the weir followed the top surface of the crest. All the quantities of leakage were corrected in this manner, where the weir height was less than 0.25 feet. Above that height, Francis's formula was assumed to give the correct discharge.

Description of Plates.

Plate I.	Fig. 1.	Section of lock, showing the arrangements for the experiments.
	Fig. 2.	Plan of lock.
Plate II.	Fig. 3.	Vertical section through 2 feet aperture.
	" 4.	Horizontal " " " "
	" 5.	Vertical " " 1 foot "
	" 6.	Horizontal " " " "
	" 7.	Enlarged section of weir.
	" 8.	Arrangement of plates of 2 by 2 feet aperture.
	" 9.	" " 2 by 1 foot "
	" 10.	" " 2 feet by 6 in "
	" 11.	Plate with 2 feet round aperture.
	" 12.	" " 1 foot square "
	" 13.	" " 1 " round "
	" 14.	" " 6 inches " "
	" 15.	Curved approach to 1 foot square aperture.
	" 16.	Hook gauge used at weir.

Experiment No. 1.

	Time.	Weir gauge.	Gauge No. 1. Centre of aperture.
	H. M.		
	9 4	.615	.853
	5	.613	.849
b	6	.610	.844
	7	.610	.844
	8		
	9	.607	.841
	10	.610	.839
	11	.608	.836
	12	.609	.831
c	13	.605	.830
	14	.605	.829
	15	.607	.830
a	16	.607	.828
	17	.607	.829

Size of aperture, 2 feet horizontal by 1.99975 feet vertical.
Aperture not filled.
Centre of aperture above weir, 1.90 feet.
Length of measuring weir, 10 feet.
Velocity of approach, 0.16 feet per second.
Head due to velocity, 0.0004 feet.
Temperature of water, 73° Fahr.
Height of water on weir for leakage, 0.05 feet.

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.......	8.28	—.70	7.58
a	8.26	—.70	7.56
b	8.29	—.70	7.59
c	8.26	—.70	7.56

	Weir gauge.	Gauge No. 1. Centre of aperture.	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on bottom of aperture.
Mean.........	.6087	.8372	937.30	—.43	936.87	.6085	1.8372
a	.6070	.8290			933.41	.6070	1.8290
b	.6100	.8440			940.28	.6100	1.8440
c	.6072	.8326			933.87	.6072	1.8326

Experiment No. 2.

9 36		2.055
37	.903	2.051
38	.903	2.057
39	.903	2.069
40	.902	2.056
41	.901	2.065
42	.901	2.068
43	.908	2.069
44	.910	2.062
45	.908	2.059
46	.904	2.063
47	.906	2.064
48	.904	2.073

Size of aperture, 2 feet horizontal by 1.99975 feet vertical.
Centre of aperture above weir, 1.90 feet.
Length of measuring weir, 10 feet.
Velocity of approach, 0.28 feet per second.
Head due to velocity, 0.0012 feet.
Temperature of water, 73° Fahr.
Height of water on weir for leakage, 0.06 feet.

Experiment No. 2.—(*Continued.*)

	Time.	Weir gauge.	Gauge No. 1. Centre of aperture.
	H. M.		
	9 49	.906	2.079
	50	.904	2.078
	51	.904	2.072
a	52	.909	2.075
a	53	.910	2.071
a	54	.908	2.072
a	55	.909	2.069
a	56	.907	
Mean.........		.9055	2.0663
a....		.9090	2.0718

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.......	11.00	—.95	10.05
a....	11.00	—.95	10.05

	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean.........	1690.40	.25	1690.65	.9056	2.0663
a....			1700.10	.9090	2.0718

Experiment No. 3.

	Time.	Weir gauge.	Gauge No. 1. Centre of aperture.
	10 24	1.023	3.019
	25	1.021	3.031
a	26	1.017	3.038
a	27	1.017	3.036
a	28	1.018	3.040
a	29	1.017	3.034
	30	1.020	3.043
c	31	1.022	3.050
c	32	1.017	3.052
c	33		
c	34	1.021	3.049
c	35	1.018	3.050
c	36	1.020	3.054
b	37	1.020	3.063
b	38	1.019	3.061
b	39	1.018	3.059
b	40	1.025	3.061
b	41	1.020	3.058

Size of aperture, 2 feet horizontal by 1.99975 feet vertical.
Centre of aperture above weir, 1.90 feet.
Length of measuring weir, 10 feet.
Velocity of approach, 0.33 feet per second.
Head due to velocity, 0.0017 feet.
Temperature of water, 74° Fahr.
Height of water on weir for leakage, 0.06 feet.

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean	12.27	—.88	11.39
a	12.25	—.88	11.37
b	12.29	—.88	11.41
c	12.27	—.88	11.39

Experiment No. 3.—(*Continued.*)

Time.	Weir gauge.	Gauge No. 1. Centre of aperture.	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean.........	1.0196	3.0469	2015.08	—.13	2014.95	1.0196	3.0469
a	1.0172	3.0370			2008.22	1.0172	3.0370
b	1.0204	3.0604			2017.41	1.0204	3.0604
c ...	1.0196	3.0510	2015.08	—.30	2014.78	1.0195	3.0510

Experiment No. 4.

	H. M.		
b	11 6	1.074	3.533
	7	1.081	3.546
	8	1.080	3.537
	9	1.073	3.542
	10	1.079	3.541
	11	1.075	3.536
	12	1.079	3.536
	13	1.085	3.532
a	14	1.086	3.537
	15	1.081	3.541
	16	1.084	3.546
	17	1.085	3.542
	18	1.090	3.540
	19	1.088	3.545
	20	1.085	3.548
	21	1.085	3.550

Size of aperture, 2 feet horizontal by 1.99975 feet vertical.
Centre of aperture above weir, 1.90 feet.
Length of measuring weir, 10 feet.
Velocity of approach, 0.36 feet per second.
Head due to velocity, 0.0020 feet.
Temperature of water, 74° Fahr.
Height of water on weir for leakage, 0.06 feet.

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.......	12.80	—.91	11.89
a	12.80	—.91	11.89
b	12.80	—.91	11.89

			Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean.........	1.0819	3.5408	2199.76	.55	2200.31	1.0821	3.5408
a	1.0855	3.5436	2210.58	—.11	2210.47	1.0855	3.5436
b	1.0782	3.5379	2188.66	1.21	2189.87	1.0786	3.5379

Experiment No. 5.

8 9	.283	.079
10	.284	.079
11	.285	.080
12	.285	.0805
13	.285	.080
14	.285	.0795

Size of aperture, 2 feet horizontal by 1 foot vertical.
Centre of aperture above weir, 2.40 feet.
Length of measuring weir, 6 feet.
Velocity of approach, 0.03 feet per second.
Head due to velocity, 0.0000 feet.
Temperature of water, 75° Fahr.
Height of water on weir for leakage, 0.05 feet.
Aperture not filled.

Experiment No. 5.—(*Continued.*)

Time.	Weir gauge.	Gauge No. 1. Centre of aperture.
H. M.		
8 15	.284	.0795
16	.285	.0795
17	.285	.079
18	.285	.079
19	.285	.0785
20	.285	.078
Mean	.2847	.0793

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.......	3.98	—.16	3.82

Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on bottom of aperture.
180 38	.14	180.52	.2849	.5793

Experiment No. 6.

Time.	Weir gauge.	Gauge No. 1. Centre of aperture.
8 31	.377	.283
32	.377	.285
33	.3775	.2854
34	.378	.2855
35	.378	.2852
36	.378	.285
37	.378	.285
38	.378	.2849
39	.377	.2849
40	.378	.285
41	.378	.285
42	.378	.285
Mean.........	.3777	.2849

Size of aperture, 2 feet horizontal by 1 foot vertical.
Centre of aperture above weir, 2.40 feet.
Length of measuring weir, 6 feet.
Velocity of approach, 0.05 feet per second.
Head due to velocity, 0.0000 feet.
Temperature of water, 75° Fahr.
Height of water on weir for leakage, 0.05 feet.
Aperture not filled.

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.......	4.20	—.23	3.97

Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on bottom of aperture.
274.77	.08	274.85	.3778	.7849

Experiment No. 7.

Time.	Weir gauge.	Gauge No. 1. Centre of aperture.
8 52	.480	.507
53	.4805	.507
54	.480	.5075
55	.480	.5085
56	.480	.5083
57	.480	.5070
58	.480	.5085
59	.480	.5090

Size of aperture, 2 feet horizontal by 1 foot vertical.
Centre of aperture above weir, 2.40 feet.
Length of measuring weir, 6 feet.
Velocity of approach, 0.07 feet per second.
Head due to velocity, 0.0001 feet.
Temperature of water, 75° Fahr.
Height of water on weir for leakage, 0.05 feet.
Aperture not filled.

Experiment No 7.—(*Continued.*)

Time. H. M.	Weir gauge.	Gauge No. 1. Centre of aperture.	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on bottom of aperture.
9 0	.480	.5065					
1	.480	.5085					
2	.480	.5090					
Mean.........	.4800	.5079	392.29		392.29	.4800	1.0079

Leakage, Cubic Feet per Minute.	Observed Leakage.	Correction for diff. of weir.	True leakage.
Mean.......	4.46	—.25	4.21

Experiment No. 8.

	Time.	Weir gauge.	Gauge No. 1. Centre of aperture.
	9 35	.758	1.773
	36	.758	1.779
	37	.758	1.783
	38	.759	1.786
b	39	.761	1.789
b	40	.761	1.792
b	41	.7605	1.796
b	42	.760	1.800
b	43	.7615	1.804
	44	.762	1.807
c	45	.763	1.809
c	46	.763	1.811
c	47	.763	1.811
c	48	.764	1.813
c	49	.763	1.815
	50	.765	1.816
d	51	.764	1.818
d	52	.764	1.820
d	53	.764	1.822
a	54	.765	1.824
a	55	.765	1.827
a	56	.765	1.828
a	57	.765	1.829
a	58	.765	1.831
a	59	.766	1.833
a	10 0	.766	1.835

Size of aperture, 2 feet horizontal by 1 foot vertical.
Centre of aperture above weir, 2.40 feet.
Length of measuring weir, 6 feet.
Velocity of approach, 0.13 feet per second.
Head due to velocity, 0.0003 feet.
Temperature of water, 76° Fahr.
Height of water on weir for leakage, 0.06 feet.

Leakage, Cubic Feet per Minute.	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.......	5.65	—.35	5.30
a....	5.66	—.35	5.31
b....	5.63	—.35	5.28
c....	5.65	—.35	5.30
d....	5.65	—.35	5 30

EXPERIMENT No. 8.—(*Continued.*)

Time.	Weir gauge.	Gauge No. 1. Centre of aperture.	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean.........	.7627	1.8096	778.20	.25	778.45	.7629	1.8096
a....	.7653	1.8296	782.12	.11	782.23	.7654	1.8296
b....	.7608	1.7962	775.35	.08	775.43	.7609	1.7962
c....	.7632	1.8118	778.95		778.95	.7632	1.8118
d....	.7640	1.8200	780.16		780.16	.7640	1.8200

EXPERIMENT No. 9.

	H. M.		
	10 23	.907	2.999
c	24	.908	3.006
c	25	.908	3.010
c	26	.9085	3.015
c	27	.909	3.019
c	28	.908	3.022
c	29	.910	3.026
c	30	.909	3.030
c	31	.908	3.035
b	32	.911	3.037
b	33	.911	3.040
b	34	.911	3.044
a	35	.912	3.048
a	36	.911	3.050
a	37	.912	3.051
a	38	.912	3.051
a	39	.912	3.053
a	40	.912	3.054

Size of aperture, 2 feet horizontal by 1 foot vertical.
Centre of aperture above weir, 2.40 feet.
Length of measuring weir, 6 feet.
Velocity of approach, 0.17 feet per second.
Head due to velocity, 0.0004 feet.
Temperature of water, 76° Fahr.
Height of water on weir for leakage, 0.06 feet.

LEAKAGE, CUBIC FEET PER MINUTE.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean	6.30	—.39	5.91
a....	6.31	—.39	5.92
b....	6.30	—.39	5.91
c....	6.29	—.39	5.90

			Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean.........	.9100	3.0328	1009.09	.22	1009.31	.9101	3.0328
a....	.9118	3.0512	1012.02		1012.02	.9118	3.0512
b....	.9110	3.0403	1010.72		1010.72	.9110	3.0403
c....	.9086	3.0204	1006.81		1006.81	.9086	3.0204

Experiment No. 10 A.

Size of aperture 2 feet horizontal by 1 foot vertical.
Centre of aperture above weir, 2.40 feet.
Length of measuring weir, 6 feet.
Velocity of approach, 0.20 feet per second.
Head due to velocity, 0.0006 feet.
Temperature of water, 76° Fahr.
Height of water on wier for leakage, 0.07 feet.

	Time. H. M.	Weir gauge.	Gauge No. 1. Centre of aperture.
	11 5	1.050	4.629
	6	1.050	4.635
	7	1.051	4.640
b	8	1.0515	4.645
b	9	1.052	4.648
b	10	1.052	4.653
b	11	1.052	4.656
b	12	1.052	4.660
b	13	1.0525	4.662
	14	1.053	4.667
a	15	1.054	4.671
a	16	1.055	4.672
a	17	1.055	4.673
a	18	1.054	4.674
a	19	1.053	4.676
a	20	1.053	4.679
a	21	1.052	4.681
a	22	1.054	4.685

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.........	7.78	—.39	7.39
a.....	7.80	—.39	7.41
b....	7.76	—.39	7.37

	Weir gauge.	Gauge No. 1. Centre of aperture.	Discharge cu. ft per minute.	Correct'n for diff. of level	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean.........	1.0526	4.6614	1249.18	.19	1249.37	1.0527	4.6614
a....	1.0537	4.6764	1251.10		1251.10	1.0537	4.6764
b.....	1.0520	4.6540	1248.14	.13	1248.27	1.0521	4.6540

Experiment No. 10 B.

Size of aperture 2 feet horizontal by 1 foot vertical.
Centre of aperture above wier, 2.40 feet.
Length of measuring weir, 10 feet.
Velocity of approach, 0.22 feet per second.
Head due to velocity, 0.0008 feet.
Temperature of water, 76° Fahr.
Height of water on weir for leakage, 0.07 feet.

	Time. H. M.	Weir gauge.	Gauge No. 1. Centre of aperture.
c	11 27	.740	4.687
c	28	.740	4.688
c	29	.740	4.689
c	30	.739	4.688
c	31	.740	4.689
c	32	.740	4.693

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
c.....	7.81	—.26	7.55

	Weir gauge.	Gauge No. 1. Centre of aperture.	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
c.....	.7398	4.6890	1252.54		1252.54	.7398	4.6890

Experiment No. 11.

Size of aperture 2 feet horizontal by 1 foot vertical.
Centre of aperture above weir, 2.40 feet.
Length of measuring weir, 10 feet.
Velocity of approach, 0.23 feet per second.
Head due to velocity, 0.0008 feet.
Temperature of water, 76° Fahr.
Height of water on weir for leakage, 0.08 feet.

Time.		Weir gauge.	Gauge No. 2. Bottom of aperture.
	H. M.		
	11 47		
d	48	.784	6.100
d	49	.785	6.110
d	50	.785	6.125
d	51	.787	6.130
d	52	.785	6.140
d	53	.786	6.145
b	54	.787	6.155
b	55	.787	6.160
b	56	.787	6.170
b	57	.787	6.175
b	58	.787	6.180
b	59	.787	6.190
c	12 0	.788	6.195
c	1	.790	6.200
c	2	.789	6.205
a	3	.790	6.210
a	4	.790	6.210
a	5	.790	6.210

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.........	9.65	—.23	9.42
a	9.70	—.23	9.47
b.....	9.65	—.23	9.42
c.....	9.69	—.23	9.46
d	9.60	—.23	9.37

	Weir gauge.	Gauge No. 2. Bottom of aperture.	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge	Corrected weir height.	Head on centre of aperture.
Mean.........	.7873	6.1667	1373.77	.27	1374.04	.7874	5.6667
a	.7900	6.2100	1380.76		1380.76	.7900	5.7100
b	.7870	6.1717	1372.99		1372.99	.7870	5.6717
c.....	.7890	6.2000	1378.17	.25	1378.42	.7891	5.7000
d	.7853	6.1250	1368.69	.25	1368.94	.7854	5.6250

Experiment No. 12.

Size of aperture, 2 feet horizontal by 1 foot vertical.
Centre of aperture above weir, 2.40 feet.
Length of measuring weir, 10 feet.
Velocity of approach, 0.26 feet per second.
Head due to velocity, 0.0011 feet.
Temperature of water, 76° Fahr.
Height of water on weir for leakage, 0.08 feet.

1 20	.842	7.370
21	.843	7.365
22	.843	7.370
23	.841	7.370
24	.840	7.370
25	.841	7.370
26	.838	7.370
27	.840	7.370

EXPERIMENT No. 12.—(*Continued.*)

Time.	Weir gauge.	Gauge No. 2. Bottom of aperture.
H. M.		
a { 1 28	.842	7.365
29	.842	7.365
30	.842	7.365
31	.841	7.360
Mean.........	.8412	7.3675
a.....	.8420	7.3650

LEAKAGE, CUBIC FEET PER MINUTE.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.........	11.15	—.27	10.88
a.....	11.15	—.27	10.88

	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge	Corrected weir height.	Head on centre of aperture.
Mean.........	1515.56	—.07	1515.49	.8412	6.8675
a.....	1517.70		1517.70	.8420	6.8650

EXPERIMENT No. 13.

1 52	.875	8.170
53	.875	8.175
54	.875	8.180
55	.874	8.185
56	.874	8.187
57	.873	8.190
58	.874	8.192
59	.874	8.194
2 0	.873	8.190
1	.873	8.193
2	.875	8.194
Mean.........	.8741	8.1864

Size of aperture, 2 feet horizontal by 1 foot vertical.
Centre of aperture above weir, 2.40 feet.
Length of measuring weir, 10 feet.
Velocity of approach, 0.27 feet per second.
Head due to velocity, 0.0011 feet.
Temperature of water, 76° Fahr.
Height of water on weir for leakage, 0.09 feet.

LEAKAGE, CUBIC FEET PER MINUTE.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.........	12.37	—.26	12.11

	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge	Corrected weir height.	Head on centre of aperture.
Mean.........	1604.27		1604.27	.8741	7.6864

EXPERIMENT No. 14.

2 14	.905	8.950
15	.904	8.956
16	.904	8.960
17	.905	8.970
18	.905	8.975
19	.904	8.980
20	.907	8.985
21	.905	8.990
22	.905	8.995
23	.905	9.000

Size of aperture, 2 feet horizontal by 1 foot vertical.
Centre of aperture above weir, 2.40 feet.
Length of measuring weir, 10 feet.
Velocity of approach, 0.28 feet per second.
Head due to velocity, 0.0012 feet.
Temperature of water, 76° Fahr.
Height of water on weir for leakage, 0.09 feet.

LEAKAGE, CUBIC FEET PER MINUTE.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.........	13.48	—.28	13.20

EXPERIMENT No. 14—(*Continued*).

Time.	Weir gauge.	Gauge No.2. Bottom of aperture.	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean.........	.9049	8.9761	1688.74		1688.74	.9049	8.4761

EXPERIMENT No. 15.

Time.	Weir gauge.	Gauge No.2. Bottom of aperture.
H. M. 2 40	.945	10.130
41	.943	10.135
42	.946	10.140
43	.946	10.140
44	.948	10.145
45	.946	10.150
a 46	.947	10.150
a 47	.949	10.155
a 48	.945	10.155
a 49	.948	10.155
a 50	.947	10.155
Mean.........	.9465	10.1464
a	.9472	10.1540

Size of aperture, 2 feet horizontal by 1 foot vertical.
Centre of aperture above weir, 2.40 feet.
Length of measuring weir, 10 feet.
Velocity of approach, 0.30 feet per second.
Head due to velocity, 0.0014 feet.
Temperature of water, 76° Fahr.
Height of water on weir for leakage, 0.10 feet.

LEAKAGE, CUBIC FEET PER MINUTE.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.......	15.49	—.25	15.24
a	15.49	—.25	15.24

	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean.........	1804.99	.15	1805.14	.9466	9.6464
a	1806.97		1806.97	.9472	9.6540

EXPERIMENT No. 16.

Time.	Weir gauge.	Gauge No.2. Bottom of aperture.
3 2	1.005	11.805
3	1.003	11.810
4	1.004	11.815
5	1.001	11.820
a 6	1.005	11.815
a 7	1.008	11.815
a 8	1.007	11.815
a 9	1.007	11.815
a 10	1.006	11.815
a 11	1.005	11.815
Mean.........	1.0051	11.8140
a	1.0063	11.8150

Size of aperture, 2 feet horizontal by 1 foot vertical.
Centre of aperture above weir, 2.40 feet.
Length of measuring weir, 10 feet.
Velocity of approach, 0.33 feet per second.
Head due to velocity, 0.0017 feet.
Temperature of water, 76° Fahr.
Height of water on weir for leakage, 0.12 feet.

LEAKAGE, CUBIC FEET PER MINUTE.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.......	20.45	—.25	20.20
a	20.45	—.25	20.20

	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean.........	1972.83		1972.83	1.0051	11.3140
a	1976.31		1976.31	1.0063	11.3150

Experiment No. 17.

Time.		Weir gauge.	Gauge No. 1. Centre of aperture.
	H. M.		
	4 59	.510	1.425
	5 0	.510	1.426
a	1	.509	1.424
a	2	.510	1.424
a	3	.510	1.424
a, b	4	.509	1.423
a, b	5	.510	1.423
a, b	6	.509	1.423
a, b	7	.509	1.423

Size of aperture, 2 feet horizontal by 0.5 feet vertical.
Centre of aperture above weir, 2.15 feet.
Length of measuring weir, 5 feet.
Velocity of approach, 0.06 feet per second.
Head due to velocity, 0.0001 feet.
Temperature of water, 76° Fahr.
Height of water on weir for leakage, 0.05.

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean......	5.19	—.26	4.93
a......	5.19	—.26	4·93
b......	5.19	—.26	4.93

	Weir gauge.	Gauge No. 1. Centre of aperture.	Discharge cu. ft. per second.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean........	.5096	1.4239	356.01	—.10	355.91	.5095	1.4239
a......	.5094	1.4220	355.81		355.81	.5094	1.4220
b......	.5092	1.4230	355.60		355.60	.5092	1.4230

Experiment No. 18.

b	5 34	.640	2.838
b	35	.643	2.850
b	36	.643	2.861
b	37	.645	2.871
b	38	.646	2.880
b	39	.646	2.888
b	40	.646	2.897
b	41	.647	2.902
b	42	.649	2.907
b	43	.648	2.912
a	44	.649	2.918
a	45	.649	2.923
a	46	.649	2.928
a	47	.649	2.932
a	48	.649	2.935
a	49	.649	2.937
a	50	.649	2.939
a	51	.650	2.940

Size of aperture, 2 feet horizontal by 0.5 feet vertical.
Centre of aperture above weir, 2.15 feet.
Length of measuring weir, 5 feet.
Velocity of approach, 0.09 feet per second.
Head due to velocity, 0.0001 feet.
Temperature of water, 76° Fahr.
Height of water on weir for leakage, 0.06 feet.

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
a....	5.94	—.29	5.65
b....	5.89	—.29	5.60

EXPERIMENT No. 18.—(*Continued.*)

Time.	Weir gauge.	Gauge No. 1. Centre of aperture.	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
a	.6490	2.9303	508.75		508.75	.6490	2.9303
b	.6453	2.8806	504.48	.76	505.24	.6460	2.8806

EXPERIMENT No. 19.

H. M.		
6 18	.762	4.742
19	.762	4.744
20	.762	4.745
21	.7615	4.746
22	.762	4.747
23	.763	4.748
24	.762	4.748
25	.763	4.747
26	.764	4.747
27	.762	4.746

Size of aperture, 2 feet horizontal by 0.5 feet vertical.
Centre of aperture above weir, 2.15 feet.
Length of measuring weir, 5 feet.
Velocity of approach, 0.11 feet per second,
Head due to velocity, 0.0002 feet.
Temperature of water, 76° Fahr.
Height of water on weir for leakage, 0.07 feet.

LEAKAGE, CUBIC FEET PER MINUTE.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean	7.60	—.29	7.31

	Weir gauge.	Gauge No. 1.	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean	.7623	4.7460	644.62		644.62	.7623	4.7460

EXPERIMENT No. 20.

	Time.	Weir gauge.	Gauge No. 2 bottom of aperture.
	7 45	.840	6.540
	46	.840	6.552
c	47	.842	6.563
c	48	.842	6.573
c	49	.842	6.582
	50	.844	6.591
b	51	.843	6.600
b	52	.842	6.608
b	53	.843	6.614
b	54	.843	6.620

Size of aperture, 2 feet horizontal by 0.5 feet vertical.
Centre of aperture above weir, 2.15 feet.
Length of measuring weir, 5 feet.
Velocity of approach, 0.13 feet per second.
Head due to velocity, 0.0003 feet.
Temperature of water, 76° Fahr.
Height of water on weir for leakage, 0.08 feet.

EXPERIMENT No. 20.—(*Continued.*)

	Time. H. M.	Weir gauge.	Gauge No. 2. Bottom of aperture.
a	7 55	.845	6.625
a	56	.844	6.629
a	57	.845	6.633
a	58	.846	6.637
a	59	.846	6.641
a	8 0	.845	6.644
a	1	.846	6.646
	a	.8452	6.6348
	b	.8427	6.6105
	c	.8429	6.5727

LEAKAGE, CUBIC FEET PER MINUTE.

	Observed leakage.	Correction for diff. of weir.	True leakage.
a	10.18	—.27	9.91
b	10.16	—.27	9.89
c	9.82	—.27	9.82

	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
a	749.00		749.00	.8452	6.3848
b	746.83		746.83	.8427	6.3605
c	745.85		745.85	.8420	6.3227

EXPERIMENT No. 21.

	Time. H. M.	Weir gauge.	Gauge No. 2. Bottom of aperture.
	8 28	.932	8.750
	29	.931	8.755
	30	.933	8.765
	31	.931	8.762
a	32	.932	8.778
a	33	.931	8.785
a	34	.932	8.790
a	35	.933	8.795
a	36	.932	8.800
a	37	.932	8.805
a	38	.932	8.810
	39	.933	8.812
	40	.934	8.816
	41	.934	8.820
	Mean	.9323	8.7895
	a	.9320	8.7947

Size of aperture, 2 feet horizontal by 0.5 feet vertical.
Centre of aperture above weir, 2.15 feet.
Length of measuring weir, 5 feet.
Velocity of approach, 0.14 feet per second.
Head due to velocity, 0.0003 feet.
Temperature of water, 76° Fahr.
Height of water on weir for leakage, 0.09 feet.

LEAKAGE, CUBIC FEET PER MINUTE.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean	13.20	—.29	12.91
a	13.20	—.29	12.91

	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean	865.75	.12	865.87	.9324	8.5395
a	865.34		865.34	.9320	8.5447

Experiment No. 22 A.

Size of aperture, 2 feet horizontal by 0.5 feet vertical.
Centre of aperture above weir, 2.15 feet.
Length of measuring weir, 5 feet.
Velocity of approach, 0.15 feet per second.
Head due to velocity, 0.0003 feet.
Temperature of water, 76° Fahr.
Height of water on weir for leakage, 0.10 feet.

Time.	Weir gauge.	Gauge No. 2. Bottom of aperture.	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
H. M.							
8 56	.968	9.810					
57	.969	9.830					
58	.971	9.840					
59	.970	9.855					
9 0	.972	9.868					
1	.971	9.880					
2	.973	9.888					
3	.972	9.898					
a 4	.972	9.905					
5	.973	9.910					
6	.9715	9.915					
7	.972	9.920					
8	.973	9.923					
Mean.........	.9714	9.8802	919.28	.30	919.58	.9716	9.6302
a.....	.9721	9.8980	920.24		920.24	.9721	9.6480

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.........	15.00	—.25	14.75
a.....	15.04	—.25	14.79

Experiment No. 22 B.

Size of aperture, 2 feet horizontal by 0.5 feet vertical.
Centre of aperture above weir, 2.15 feet.
Length of measuring weir, 5 feet.
Velocity of approach, 0.17 feet per second.
Head due to velocity, 0.0004 feet.
Temperature of water, 76° Fahr.
Height of water on weir for leakage, 0.12 feet.

Time.	Weir gauge.	Gauge No. 2. Bottom of aperture.	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
9 28	1.036	11.775					
29	1.036	11.785					
30	1.036	11.795					
31	1.036	11.800					
32	1.036	11.805					
33	1.035	11.810					
34	1.035	11.812					
35	1.036	11.814					
36	1.037	11.816					
37	1.035	11.818					
38	1.035	11.820					
39	1.036	11.820					
Mean.........	1.0358	11.8058	1009.49		1009.49	1.0358	11.5558

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.........	20.50	—.28	20.22

Experiment No. 22 C.

Time.	Weir gauge.	Gauge No. 2. Bottom of aperture.
H. M.		
9 43	.640	11.822
44	.639	11.822
45	.640	11.825
46	.638	11.822
47	.638	11.823
Mean.........	.6390	11.8228

Size of aperture, 2 feet horizontal by 0.5 feet vertical.
Centre of aperture above weir, 2.15 feet.
Length of measuring weir, 10 feet.
Velocity of approach, 0.18 feet per second.
Head due to velocity, 0.0005 feet.
Temperature of water, 76° Fahr.
Height of water on weir for leakage, 0.12 feet.

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.........	20.45	—.12	20.33

Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
1007.53		1007.53	.6390	11.5728

Experiment No. 23 A.

10 1	.677	13.738
2	.676	13.745
3	.676	13.750
4	.677	13.755
5	.678	13.758
6	.678	13.760
7	.677	13.763
8	.676	13.763
9	.678	13.763
10	.678	13.761
11	.676	13.761
12	.675	13.761
Mean.........	.6770	13.7565

Size of aperture, 2 feet horizontal by 0.5 feet vertical.
Centre of aperture above weir, 2.15 feet.
Length of measuring weir, 10 feet.
Velocity of approach, 0.19 feet per second.
Head due to velocity, 0.0006 feet.
Temperature of water, 76° Fahr.
Height of water on weir for leakage, 0.16 feet.

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.........	30.90	—.14	30.76

Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
1097.88		1097.89	.6770	13.5065

Experiment No. 23 B.

10 24	.704	15.295
25	.704	15.300
26	.705	15.303
27	.706	15.305
28	.704	15.305

Size of aperture, 2 feet horizontal by 0.5 feet vertical.
Centre of aperture above weir, 2.15 feet.
Length of measuring weir, 10 feet.
Velocity of approach, 0.20 feet per second.
Head due to velocity, 0.0006 feet.
Temperature of water, 76° Fahr.
Height of water on weir for leakage, 0.21 feet.

EXPERIMENT No. 23 B.—(*Continued.*)

Time.		Weir gauge.	Gauge No. 2. Bottom of aperture.
	H. M.		
a	10 29	.705	15.308
a	30	.706	15.308
a	31	.705	15.308
a	32	.706	15.310
a	33	.705	15.315
	34	.706	15.315

LEAKAGE, CUBIC FEET PER MINUTE.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean	43.83	—.11	43.72
a	43.83	—.11	43.72

	Weir gauge.	Gauge No. 2. Bottom of aperture.	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean	.7051	15.3065	1166.29	.15	1166.44	.7052	15.0565
a	.7054	15.3098	1167.02		1167.02	.7054	15.0598

EXPERIMENT No. 24.

Time.		Weir gauge.	Gauge No. 2. Bottom of aperture.
	10 42	.739	17.215
	43	.740	17.215
	44	.740	17.213
	45	.735	17.213
a	46	.736	17.215
a	47	.737	17.213
a	48	.737	17.215
a	49	.737	17.217
a	50	.738	17.217
a	51	.736	17.215
a	52	.737	17.217
a	53	.736	17.217

Size of aperture, 2 feet horizontal by 0.5 feet vertical.
Centre of aperture above weir, 2.15 feet.
Length of measuring weir, 10 feet.
Velocity of approach, 0.21 feet per second.
Head due to velocity, 0.0007 feet.
Temperature of water, 76° Fahr.
Height of water on weir for leakage, 0.28 feet.

LEAKAGE, CUBIC FEET PER MINUTE.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.......	57.30	—.12	57.18
a	57.30	—.12	57.18

	Weir gauge.	Gauge No. 2. Bottom of aperture.	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean.........	.7373	17.2152	1246.26	—.23	1246.03	.7372	16.9652
a	.7367	17.2157	1244.76		1244.76	.7367	16.9657

EXPERIMENT No. 25.

Time.	Weir gauge.	Gauge No. 1. Centre of aperture.
2 7	.710	1.757
8	.710	1.753
9	.711	1.755
10	.711	1.758
11	.712	1.765

Size of aperture, round, 2 feet diameter.
Centre of aperture above weir, 1.97 feet.
Length of measuring weir, 10 feet.
Velocity of approach, 0.21 feet per second.
Head due to velocity, 0.0007 feet.
Temperature of water, 76° Fahr.
Height of water on weir for leakage, 0.05 feet.

Experiment No. 25.—(*Continued.*)

	Time.	Weir gauge.	Gauge No. 1. Centre of aperture.
	H. M. 2 12	.712	1.766
	13	.712	1.771
	14	.714	1.775
a	15	.715	1.775
a	16	.713	1.775
a	17	.715	1.775
a	18	.715	1.778
a	19	.716	1.777

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.......	5.35	—.35	5.00
a	5.36	—.35	5.01

	Weir gauge.	Gauge No. 1. Centre of aperture.	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean.........	.7128	1.7677	1185.20	.38	1185.58	.7130	1.7677
a	.7148	1.7760	1190.18	.19	1190.37	.7149	1.7760

Experiment No. 26.

	Time.	Weir gauge.	Gauge No. 1. Centre of aperture.
	2 35	.817	2.582
	36	.813	2.584
	37	.815	2.586
	38	.815	2.587
	39	.815	2.590
	40	.816	2.595
a	41	.816	2.600
a	42	.818	2.604
a	43	.817	2.606
a	44	.817	2.606
a	45	.816	2.604
a	46	.816	2.601
a	47	.816	2.600

Size of aperture, round, 2 feet diameter.
Centre of aperture above weir, 1.97 feet.
Length of measuring weir, 10 feet.
Velocity of approach, 0.25 feet per second.
Head due to velocity, 0.0010 feet.
Temperature of water, 76° Fahr.
Height of water on weir for leakage, 0.06 feet.

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.......	5.78	—.39	5.39
a	5.78	—.39	5.39

	Weir gauge.	Gauge No. 1. Centre of aperture.	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean	.8159	2.5958	1448.46	.06	1448.52	.8159	2.5958
a	.8166	2.6030	1450.30		1450.30	.8166	2.6030

Experiment No. 27.

Time.	Weir gauge.	Gauge No. 1. Centre of aperture.
3 3	.990	4.430
4	.990	4.438
5	.990	4.444

Size of aperture, round, 2 feet diameter.
Centre of aperture above weir, 1.97 feet.
Length of measuring weir, 10 feet.
Velocity of approach, 0.32 feet per second.
Head due to velocity, 0.0016 feet.
Temperature of water, 75° Fahr.
Height of water on weir for leakage, 0.07 feet.

EXPERIMENT No. 27.—(*Continued.*)

	Time.	Weir gauge.	Gauge No. 1. Centre of aperture.
	H. M. 5 6	.988	4.445
a	7	.991	4.463
a	8	.991	4.470
a	9	.988	4.477
a	10	.990	4.482
a	11	.992	4.488
a	12	.990	4.491
a	13	.990	4.495
a	14	.990	4.496
a	15	.991	4.502
a	16	.994	4.508

LEAKAGE, CUBIC FEET PER MINUTE.

	Observed leakage.	Correction for diff of weir.	True leakage.
Mean.......	7.15	—.40	6.75
a	7.16	—.40	6.76

	Weir gauge.	Gauge No. 1. Centre of aperture.	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean.........	.9904	4.4735	1930.29	.23	1930.52	.9905	4.4735
a	.9903	4.4849	1930.00		1930.00	.9903	4.4849

EXPERIMENT No. 28.

Size of aperture, round, 2 feet diameter.
Centre of aperture above weir, 1.97 feet.
Length of measuring weir, 10 feet.
Velocity of approach, 0.36 feet per second.
Head due to velocity, 0.0020 feet.
Temperature of water, 75° Fahr.
Height of water on weir for leakage, 0.07 feet.

	Time.	Weir gauge.	Gauge No. 2 Centre of aperture.
	3 36	1.093	5.800
a	37	1.090	5.810
a	38	1.090	5.815
a	39	1.090	5.820
a	40	1.093	5.825
a	41	1.090	5.830
a	42	1.091	5.835
a	43	1.090	5.840
a	44	1.091	5.840
b	45	1.095	5.850
b	46	1.094	5.855
b	47	1.093	5.855
b	48	1.097	5.860

LEAKAGE, CUBIC FEET PER MINUTE.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.......	9.26	—.35	8.91
a	9.25	—.35	8.90
b	9.30	—.35	8.95

	Weir gauge.	Gauge No. 2 Centre of aperture.	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean.........	1.0921	5.8335	2230.48	.25	2230.73	1.0922	5.8335
a	1.0906	5.8269	2225.95	.11	2226.06	1.0906	5.8269
b	1.0947	5.8550	2238.33	.51	2238.84	1.0949	5.8550

EXPERIMENT No. 29.

Time. H. M.		Weir gauge.	Gauge No. 2. Centre of aperture.
	4 2	1.156	6.880
b	3	1.160	6.890
b	4	1.163	6.900
b	5	1.157	6.915
b	6	1.160	6.930
a	7	1.162	6.940
a	8	1.161	6.945
a	9	1.167	6.950
a	10	1.158	6.955
a	11	1.164	6.960
a	12	1.159	6.965
a	13	1.160	6.970
Mean.........		1.1606	6.9333
a....		1.1614	6.9519
b....		1.1600	6.9087

Size of aperture, round, 2 feet diameter.
Centre of aperture above weir, 1.97 feet.
Length of measuring weir, 10 feet.
Velocity of approach, 0.39 feet per second.
Head due to velocity, 0.0024 feet.
Temperature of water, 75° Fahr.
Height of water on weir for leakage, 0.08 feet.

LEAKAGE, CUBIC FEET PER MIMUTE.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.......	10.62	—.38	10.24
a....	10.64	—.38	10.26
b....	10.60	—.38	10.22

	Discharge cu.ft. per minute.	Corre't'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean	2440.17	.28	2440.45	1.1607	6.9333
a	2442.65		2442.65	1.1614	6.9519
b	2438.31		2438.31	1.1600	6.9087

EXPERIMENT No. 30.

Time. H. M.		Weir gauge.	Gauge No. 2. Centre of aperture.
	4 27	1.235	8.300
	28	1.232	8.315
	29	1.233	8.325
	30	1.232	8.330
	31	1.237	8.340
a	32	1.236	8.345
a	33	1.237	8.355
a	34	1.237	8.360
a	35	1.240	8.365
a	36	1.238	8.370
a	37	1.236	8.370
Mean.........		1.2357	8.3432
a....		1.2373	8.3608

Size of aperture, round, 2 feet diameter.
Centre of aperture above weir, 1.97 feet.
Length of measuring weir, 10 feet.
Velocity of approach, 0.43 feet per second.
Head due to velocity, 0.0029 feet.
Temperature of water, 75° Fahr.
Height of water on weir for leakage, 0.09 feet.

LEAKAGE, CUBIC FEET PER MINUTE.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.......	12.67	—.38	12.29
a....	12.70	—.38	12.32

	Discharge cu.ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean	2676.68	.08	2676.76	1.2357	8.3432
a	2681.79		2681.79	1.2373	8.3608

Experiment No. 31.

Time.	Weir gauge.	Gauge No. 2. Centre of aperture.
H. M.		
4 49	1.302	9.60
50	1.304	9.61
51	1.298	9.62
52	1.300	9.63
53	1.298	9.64
54	1.305	9.655
55	1.309	9.67
56	1.302	9.68
Mean.........	1.3022	9.6381

Size of aperture, round, 2 feet diameter.
Centre of aperture above weir, 1.97 feet.
Length of measuring weir, 10 feet.
Velocity of approach, 0.46 feet per second.
Head due to velocity, 0.0033 feet.
Temperature of water, 75° Fahr.
Height of water on weir for leakage, 0.10 feet.

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.........	14.72	—.37	14.35

Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
2891.68		2891.68	1.3022	9.6381

Experiment No. 32.

	Time.	Weir gauge.	Gauge No. 1. Centre of aperture.
	H. M.		
a	4 46	.392	.514
a	47	.393	.516
a	48	.393	.516
a	49	.393	.516
a	50	.393	.516
a	51	.393	.517
a	52	.393	.516
a	53	.393	.515
	a.....	.3930	.5160

Size of aperture, 1.0000833 feet square.
Centre of aperture above weir, 1.556 feet.
Length of measuring weir, 4 feet.
Velocity of approach, 0.03 feet per second.
Head due to velocity, 0.0000 feet.
Temperature of water, 75° Fahr.
Height of water on weir for leakage, 0.08 feet.
Aperture not filled.

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
a.........	10.15	—.59	9.56

Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on bottom of aperture.
193.12		193.12	.3930	1.0160

Experiment No. 33.

b	5 13	.593	1.471
b	14	.593	1.476
b	15	.593	1.480
b	16	.593	1.484
b	17	.593	1.487
	18	.594	1.490

Size of aperture, 1.0000833 feet square.
Centre of aperture above weir, 1.556 feet.
Length of measuring weir, 4 feet.
Velocity of approach, 0.06 feet per second.
Head due to velocity, 0.0001 feet.
Temperature of water, 75° Fahr.
Height of water on weir for leakage, 0.09 feet.

Experiment No. 33.—(*Continued.*)

Time.		Weir gauge.	Gauge No. 1. Centre of aperture.
	H. M.		
	5 19	.595	1.492
	20	.594	1.494
	21	.595	1.495
a	22	.595	1.494
	23	.596	1.494
	24	.595	1.495
	25	.595	1.495
Mean.........		.5942	1.4882
a.....		.5950	1.4941
b.....		.5930	1.4796

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.........	12.21	—.76	11.45
a.....	12.21	—.76	11.45
b.....	12.20	—.76	11.44

	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean.........	355.19	.13	355.32	.5944	1.4882
a.....	355.89		355.89	.5950	1.4941
b.....	354.13		354.13	.5930	1.4796

Experiment No. 34.

Time.		Weir gauge.	Gauge No. 1. Centre of aperture.
	5 53	.812	3.631
	54	.813	3.639
	55	.814	3.648
	56	.814	3.656
	57	.815	3.664
	58	.816	3.672
	59	.817	3.679
b	6 0	.817	3.688
	1	.817	3.699
	2	.818	3.706
	3	.819	3.713
	4	.820	3.717
	5	.821	3.720
	6	.821	3.723
a	7	.819	3.722
	8	.819	3.719
	9	.819	3.716
	10	.819	3.711
Mean.........		.8172	3.6902
a....		.8196	3.7174
b....		.8170	3.6887

Size of aperture, 1.0000833 feet square.
Centre of aperture above weir, 1.556 feet.
Length of measuring weir, 4 feet.
Velocity of approach, 0.10 feet per second.
Head due to velocity, 0.0002 feet.
Temperature of water, 76° Fahr.
Height of water on weir for leakage, 0.10 feet.

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.........	14.30	—.80	13.50
a.....	14.32	—.80	13.52
b.....	14.30	—.80	13.50

	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean.........	566.28	.31	566.59	.8175	3.6902
a....	568.71		568.71	.8196	3.7174
b....	566.08		566.08	.8170	3.6887

EXPERIMENT No. 35.

Time.		Weir gauge.	Gauge No. 1. Centre of aperture.
	H. M. 6 35	.892	4.774
b	36	.893	4.780
b	37	.893	4.784
b	38	.894	4.785
b	39	.893	4.786
b	40	.893	4.786
b	41	.893	4.787
b	42	.894	4.792
b	43	.893	4.795
b	44	.893	4.799
b	45	.893	4.802
a	46	.894	4.807
a	47	.894	4.810
a	48	.894	4.817
a	49	.894	4.821
a	50	.894	4.827
	51	.895	4.832
	52	.896	4.838
	53	.896	4.846
Mean.........		.8937	4.8036
a. ...		.8940	4.8164
b.....		.8930	4.7896

Size of aperture, 1.0000833 feet square.
Centre of aperture above weir, 1.556 feet.
Length of measuring weir, 4 feet.
Velocity of approach, 0.11 feet per second.
Head due to velocity, 0.0002 feet.
Temperature of water, 76° Fahr.
Height of water on weir for leakage, 0.10 feet.

LEAKAGE, CUBIC FEET PER MINUTE.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.........	15.97	—.83	15.14
a.....	16.01	—.83	15.18
b....	15.94	—.83	15.11

	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean	645.03	.17	645.20	.8939	4.8036
a	645.35		645.35	.8940	4.8164
b	644.30		644.30	.8930	4.8196

EXPERIMENT No. 36.

Time.	Weir gauge.	Gauge No. 2. Centre of aperture.
H. M. 7 18	.933	5.495
19	.933	5.493
20	.933	5.492
21	.933	5.491
22	.935	5.490
23	.934	5.490
24	.934	5.490
25	.935	5.490

Size of aperture, 1.0000833 feet square.
Centre of aperture above weir, 1.556 feet.
Length of measuring weir, 4 feet.
Velocity of approach, 0.11 feet per second.
Head due to velocity, 0.0002 feet.
Temperature of water, 75° Fahr.
Height of water on weir for leakage, 0.11 feet.

EXPERIMENT No. 36.—(*Continued.*)

Time.	Weir gauge.	Gauge No. 2. Centre of aperture.	Leakage, Cubic Feet per Minute.				
H. M.				Observed leakage.	Correction for diff. of weir.	True leakage.	
7 26	.934	5.490					
27	.932	5.490					
28	.933	5.488	Mean.........	16.81	—.77	16.04	
			Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean.........	.9335	5.4908	687.18		687.18	.9335	5.4908

EXPERIMENT No. 37.

Size of aperture, 1.0000833 feet square.
Centre of aperture above weir, 1.556 feet.
Length of measuring weir, 4 feet.
Velocity of approach, 0.13 feet per second.
Head due to velocity, 0.0003 feet.
Temperature of water, 75° Fahr.
Height of water on weir for leakage, 0.13 feet.

Time.	Weir gauge.	Gauge No. 2. Centre of aperture.
7 48	.999	6.530
49	.999	6.565
50	1.000	6.590
51	1.001	6.615
52	1.002	6.635
53	1.004	6.655
54	1.004	6.680
55	1.005	6.705
56	1.005	6.720
57	1.007	6.735
58	1.007	6.750
59	1.008	6.763
8 0	1.008	6.775
1	1.009	6.788
2	1.009	6.800
a 3	1.010	6.810
a 4	1.010	6.817
a 5	1.010	6.823
6	1.011	6.830
7	1.012	6.836

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.........	19.24	—.76	18.48
a.....	19.46	—.76	18.70

	Weir gauge.	Gauge No. 2. Centre of aperture.	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean.........	1.0060	6.7211	765.84	.52	766.36	1.0065	6.7211
a.....	1.0100	6.8167	770.25		770.25	1.0100	6.8167

Experiment No. 38 A.

Time.		Weir gauge.	Gauge No. 2. Centre of aperture.
	H. M. 8 32	1.141	9.740
	33	1.141	9.750
b	34	1.142	9.765
b	35	1.140	9.775
b	36	1.140	9.785
b	37	1.142	9.795
b	38	1.142	9.805
	39	1.144	9.815
a	40	1.145	9.823
a	41	1.1455	9.830
a	42	1.145	9.835
	43	1.147	9.840
	44	1.148	9.845
	45	1.148	9.850

Size of aperture, 1.0000833 feet square.
Centre of aperture above weir, 1.556 feet.
Length of measuring weir, 4 feet.
Velocity of approach, 0.15 feet per second.
Head due to velocity, 0.0003 feet.
Temperature of water, 75° Fahr.
Height of water on weir for leakage, 0.15 feet.

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.......	24.72	—.74	23.98
a....	24.76	—.74	24.02
b....	24.68	—.74	23.94

	Weir gauge.	Gauge No. 2. Centre of aperture.	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean.........	1.1436	9.8038	921.50	.41	921.91	1.1440	9.8038
a....	1.1451	9.8293	923.12		923.12	1.1451	9.8293
b....	1.1412	9.7850	918.71		918.71	1.1412	9.7850

Experiment No. 38 B.

Time.		Weir gauge.	Gauge No. 2. Centre of aperture.
	8 56	.652	9.893
	57	.651	9.896
	58	.650	9.898
	59	.650	9.900
d	9 0	.652	9.902
d	1	.652	9.905
d	2	.652	9.907

Size of aperture, 1.0000833 feet square.
Centre of aperture above weir, 1.556 feet.
Length of measuring weir, 9.013 feet.
Velocity of approach, 0.16 feet per second.
Head due to velocity, 0.0004 feet.
Temperature of water, 75° Fahr.
Height of water on weir for leakage, 0.15 feet.

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.......	24.89	—.37	24.52
d....	24.89	—.37	24.52

	Weir gauge.	Gauge No. 2. Centre of aperture.	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean.........	.6513	9.9001	934.35		934.35	.6513	9.9001
d....	.6520	9.9047	935.70		935.70	.6520	9.9047

Experiment No. 39.

Time.	Weir gauge.	Gauge No. 2. Centre of aperture.
H. M. 9 31	.647	11.990
32	.647	11.995
33	.647	12.000
34	.647	12.003
35	.649	12.005
36	.648	12.007
37	.647	12.010
38	.647	12.012
39	.646	12.012
40	.647	12.012
Mean.	.6472	12.0046

Size of aperture, 1.0000833 feet square.
Centre of aperture above weir, 1.556 feet.
Length of measuring weir, 10 feet.
Velocity of approach, 0.18 feet per second.
Head due to velocity, 0.0005 feet.
Temperature of water, 75° Fahr.
Height of water on weir for leakage, 0.17 feet.

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.......	28.28	—.35	27.93

Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
1026.82		1026.82	.6472	12.0046

Experiment No. 40.

Time.	Weir gauge.	Gauge No. 2. Centre of aperture.
10 0	.678	13.615
1	.677	13.622
2	.677	13.626
3	.678	13.630
4	.675	13.633
5	.677	13.633
a { 6	.676	13.635
7	.678	13.637
8	.676	13.637
9	.676	13.635
Mean.........	.6768	13.6293
a	.6765	13.6360

Size of aperture, 1.0000833 feet square.
Centre of aperture above weir, 1.556 feet.
Length of measuring weir, 10 feet.
Velocity of approach, 0.19 feet per second.
Head due to velocity, 0.0006 feet.
Temperature of water, 75° Fahr.
Height of water on weir for leakage, 0.18 feet.

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.......	31.53	—.32	31.21
a	31.53	—.32	31.21

	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean	1097.41	—.17	1097.24	.6767	13.6293
a	1096.68		1096.68	.6765	13.6360

Experiment No. 41.

Time.		Weir gauge.	Gauge No. 2. Centre of aperture.
	H. M.		
	10 29	.7015	15.130
a	30	.703	15.130
a	31	.702	15.133
a	32	.702	15.132
a	33	.701	15.133
a	34	.704	15.132
a	35	.702	15.132
a	36	.703	15.132
a	37	.703	15 133
	38	.702	15.135

Size of aperture, 1.0000833 feet square.
Centre of aperture above weir, 1.556 feet.
Length of measuring weir, 10 feet.
Velocity of approach, 0.20 feet per second.
Head due to velocity, 0.0006 feet.
Temperature of water, 75° Fahr.
Height of water on weir for leakage, 0.19 feet.

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.......	36.35	—.31	36.04
a....	36.35	—.31	36.04

	Weir gauge.	Gauge No. 2. Centre of aperture.	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean.........	.7023	15.1322	1159.40	.04,	1159.44	.7023	15.1322
a....	.7025	15.1321	1159.89		1159.89	.7025	15.1321

Experiment No. 42.

Time.	Weir gauge.	Gauge No. 2. Centre of aperture.
10 49	.738	17.570
50	.739	17.565
51	.738	17.570
52	.7335	17.567
53	.732	17.570
54	.738	17.565
55	.734	17.563
56	.7375	17.560
57	.740	17.560
58	.738	17.557

Size of aperture, 1.0000833 feet square.
Centre of aperture above weir, 1.556 feet.
Length of measuring weir, 10 feet.
Velocity of approach, 0.21 feet per second.
Head due to velocity, 0.0007 feet.
Temperature of water, 75° Fahr.
Height of water on weir for leakage, 0.22 feet.

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.......	43.10	—.32	42.78

	Weir gauge.	Gauge No. 2. Centre of aperture.	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean..........	.7368	17.5647	1245.01		1245.01	.7368	17.5647

Experiment No. 43.

Time.		Weir gauge.	Gauge No. 1. Centre of aperture.
a	1 31	.461	1.149
a	32	.461	1.148
a	33	.461	1.147
a	34	.461	1.147

Size of aperture, 1.0007 feet diameter, round.
Centre of aperture above weir, 1.556 feet.
Length of measuring weir, 4 feet.
Velocity of approach, 0.04 feet per second.
Head due to velocity, 0.0000 feet.
Temperature of water, 75° Fahr.
Height of water on weir for leakage, 0.09 feet.

EXPERIMENT No. 43.—(*Continued.*)

	Time. H. M.	Weir gauge.	Gauge No. 1. Centre of aperture.
	1 35	.4605	1.147
	36	.4605	1.147
a	37	.460	1.147
a	38	.460	1.147
a	39	.460	1.147
a	40	.460	1.147

LEAKAGE, CUBIC FEET PER MINUTE.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.........	11.62	—.57	11.05
a	11.62	—.57	11.05
b	11.62	—.57	11.05

	Weir gauge.	Gauge No. 1. Centre of aperture.	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean.........	.4605	1.1473	243.99	—.08	243.91	.4604	1.1473
a	.4600	1.1470	243.60		243.60	.4600	1.1470
b	.4610	1.1478	244.38		244.38	.4610	1.1478

EXPERIMENT No. 44.

	Time. H. M.	Weir gauge.	Gauge No. 1. Centre of aperture.
	1 59	.588	2.309
	2 0	.590	2.324
	1	.590	2.333
	2	.593	2.344
	3	.5915	2.351
	4	.592	2.358
b	5	.594	2.364
b	6	.594	2.369
b	7	.594	2.373
a	8	.595	2.377
a	9	.595	2.379
a	10	.596	2.381
a	11	.596	2.382
a	12	.595	2.383
a	13	.595	2.384

Size of aperture, 1.0007 feet diameter, round.
Centre of aperture above weir, 1.556 feet.
Length of measuring weir, 4 feet.
Velocity of approach, 0.06 feet per second.
Head due to velocity, 0.0001 feet.
Temperature of water, 75° Fahr.
Height of water on weir for leakage, 0.09 feet.

LEAKAGE, CUBIC FEET PER MINUTE.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.........	12.24	—.66	12.58
a	13.27	—.66	12.61
b	13.26	—.66	12.60

	Weir gauge.	Gauge No. 1. Centre of aperture.	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean.........	.5932	2.3607	354.31	.38	354.69	.5937	2.3607
a	.5953	2.3810	356.15		356.15	.5953	2.3810
b	.5940	2.3687	355.01		355.01	.5940	2.3687

Experiment No. 45.

Time.		Weir gauge.	Gauge No. 1. Centre of aperture.
	H. M.		
	2 47	.753	4.761
	48	.753	4.770
	49	.754	4.779
	50	.754	4.788
a	51	.755	4.797
a	52	.755	4.804
a	53	.754	4.811
a	54	.755	4.817
a	55	.756	4.822
a	56	.755	4.828
a	57	.755	4.832
b	58	.756	4.836
b	59	.756	4.839
b	3 0	.756	4.843
Mean.........		.7548	4.8091
	a	.7550	4.8159
	b	.7560	4.8393

Size of aperture, 1.0007 feet diameter, round.
Centre of aperture above weir, 1.556 feet.
Length of measuring weir, 4 feet.
Velocity of approach, 0.09 feet per second.
Head due to velocity, 0.0001 feet.
Temperature of water, 75° Fahr.
Height of water on weir for leakage, 0.10 feet.

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.........	15.49	—.69	14.80
a	15.51	—.69	14.82
b	15.56	—.69	14.87

	Discharge cu ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean	504.30	.18	504.48	.7550	4.8091
a	504.50		504.50	.7550	4.8159
b	505.47		505.47	.7560	4.8393

Experiment No. 46 A.

Time.		Weir gauge.	Gauge No. 2. Centre of aperture.
	3 31	.902	8.025
	32	.900	8.010
	33	.901	8.000
	34	.899	7.985
	35	.899	7.975
	36	.898	7.967
a	37	.897	7.960
a	38	.897	7.954
a	39	.8965	7.949
a	40	.897	7.945
	41	.8965	7.940
	42	.895	7.936

Size of aperture, 1.0007 feet diameter, round.
Centre of aperture above weir, 1.556 feet.
Length of measuring weir, 4 feet.
Velocity of approach, 0.11 feet per second.
Head due to velocity, 0.0002 feet.
Temperature of water, 75° Fahr.
Height of water on weir of leakage, 0.13 feet.

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.........	21.70	—.62	21.08
a	21.67	—.63	21.04

Experiment No. 46 A.—(*Continued.*)

Time.	Weir gauge.	Gauge No. 2. Centre of aperture.	Discharge cu. ft. per minute.	Correc'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean.........	.8932	7.9705	644.51	—.48	644.03	.8927	7.9705
a.....	.8969	7.9520	648.40		648.40	.8969	7.9520

Experiment No. 46 B.

	H. M.		
	3 48	.476	7.922
	49	.476	7.921
	50	.475	7.920
b	51	.474	7.918
	52	.474	7.916
	53	.475	7.915
	54	.4745	7.913
	55	.477	7.913

Size of aperture, 1.0007 feet diameter, round.
Centre of aperture above weir, 1.556 feet.
Length of measuring weir, 10 feet.
Velocity of approach, 0.12 feet per second.
Head due to velocity, 0.0002 feet.
Temperature of water, 75° Fahr.
Height of water on weir for leakage, 0.13 feet.

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.........	21.60	—.28	21.32
b....	21.60	—.28	21.32

			Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean.........	.4752	7.9172	648.08	.07	648.15	.4752	7.9172
b....	.4745	7.9172	646.87		646.87	.4745	7.9172

Experiment No. 47.

	4 17	.530	10.800
	18	.530	10.820
	19	.532	10.840
b	20	.532	10.860
	21	.532	10.880
	22	.532	10.895
	23	.533	10.905
	24	.532	10.913
a	25	.533	10.921
	26	.534	10.930
	27	.533	10.937

Size of aperture, 1.0007 feet diameter, round.
Centre of aperture above weir, 1.556 feet.
Length of measuring weir, 10 feet.
Velocity of approach, 0.14 feet per second.
Head due to velocity, 0.0003 feet.
Temperature of water, 76° Fahr.
Height of water on weir for leakage, 0.16 feet.

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.........	26.38	—.26	26.12
a.....	26.42	—.26	26.16
b....	26.37	—.26	26.11

			Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean.........	.5321	10.8819	767.25	.23	767.48	.5322	10.8819
a.....	.5330	10.9212	769.19		769.19	.5330	10.9212
b....	.5320	10.8687	767.04		767.04	.5320	10.8687

Experiment No. 48.

	Time.	Weir gauge.	Gauge No. 2. Centre of aperture.
	H. M.		
	4 44	.556	12.413
	45	.556	12.430
b	46	.557	12.446
b	47	.557	12.462
b	48	.557	12.476
b	49	.557	12.485
a	50	.559	12.492
a	51	.558	12.500
a	52	.558	12.506
a	53	.557	12.515
a	54	.559	12.522

Size of aperture, 1.0007 feet diameter, round.
Centre of aperture above weir, 1.556 feet.
Length of measuring weir, 10 feet.
Velocity of approach, 0.15 feet per second.
Head due to velocity, 0.0003 feet.
Temperature of water, 76° Fahr.
Height of water on weir for leakage, 0.17 feet.

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.........	29.10	—.27	28.83
a.....	29.13	—.27	28.86
b....	29.08	—.27	28.81

	Weir gauge.	Gauge No. 2. Centre of aperture.	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean.........	.5574	12.4770	822.20	.23	822.43	.5575	12.4770
a.....	.5582	12.5070	823.96		823.96	.5582	12.5070
b....	.5570	12.4672	821.32		821.32	.5570	12.4672

Experiment No. 49.

	Time.	Weir gauge.	Gauge No. 2. Centre of aperture.
	5 10	....	....
b	11	.582	14.090
b	12	.582	14.100
b	13	.582	14.110
b	14	.582	14.120
b	15	.5825	14.127
a	16	.583	14.132
a	17	.582	14 136
a	18	.583	14.145
a	19	.583	14.152
a	20	.583	14.157

Size of aperture, 1.0007 feet diameter, round.
Centre of aperture above weir, 1.556 feet.
Length of measuring weir, 10 feet.
Velocity of approach, 0.16 feet per second.
Head due to velocity, 0.0004 feet.
Temperature of water, 76° Fahr.
Height of water on weir for leakage, 0.18 feet.

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.........	32.74	—.29	32.45
a.....	32.76	—.29	32.47
b....	32.70	—.29	32.41

	Weir gauge.	Gauge No. 2. Centre of aperture.	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean.........	.5827	14.1269	878.36	.08	878.44	.5827	14.1269
a.....	.5828	14.1444	878.58		878.58	.5828	14.1444
b....	.5821	14.1094	877.01		877.01	.5821	14 1094

Experiment No. 50.

	Time.	Weir gauge.	Gauge No. 2. Centre of aperture.
	H. M. 5 39	.605	15.655
b	40	.605	15.660
b	41	.605	15.663
b	42	.604	15.665
b	43	.605	15.665
b	44	.6045	15.665
a	45	.606	15.667
a	46	.605	15.664
a	47	.6065	15.665
a	48	.606	15.665
a	49	.605	15.667
a	50	.606	15.667
Mean.........		.6052	15.6640
a		.6057	15.6658
b		.6047	15.6636

Size of aperture, 1.0007 feet diameter, round.
Centre of aperture above weir, 1.556 feet.
Length of measuring weir, 10 feet.
Velocity of approach, 0.16 feet per second.
Head due to velocity, 0.0004 feet.
Temperature of water, 76° Fahr.
Height of water on weir for leakage, 0.20 feet.

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.......	38.25	—.24	38.01
a	38.25	—.24	38.01
b	38.25	—.24	38.01

	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean	929.30	.07	929.37	.6052	15.6640
a	930.55		930.55	.6057	15.6658
b	928.16		928.16	.6047	15.6636

Experiment No. 51.

	Time.	Weir gauge.	Gauge No. 2. Centre of aperture.
	5 57	.635	17.695
b	58	.634	17.710
b	59	.634	17.710
b	6 0	.634	17.725
b	1	.634	17.715
	2	.633	17.730
a	3	.635	17.730
a	4	.636	17.740
a	5	.635	17.745
a	6	.635	17.745
Mean.........		.6345	17.7245
a		.6352	17.7400
b		.6340	17.7150

Size of aperture, 1.0007 feet diameter, round.
Centre of aperture above weir, 1.556 feet.
Length of measuring weir, 10 feet.
Velocity of approach, 0.17 feet per second.
Head due to velocity, 0.0004 feet.
Temperature of water, 76° Fahr.
Height of water on weir for leakage, 0.23 feet.

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.......	43.40	—.24	43.16
a	43.45	—.24	43.21
b	43.38	—.24	43.14

	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean	997.00		997.00	.6345	17.7245
a	998.64		998.64	.6352	17.7400
b	995.83		995.83	.6340	17.7150

Experiment No. 52.

	Time. H. M.	Weir gauge.	Gauge No. 1. Centre of aperture.
	12 9	.384	2.174
	10	.384	2.169
	11	.383	2.164
	12	.383	2.158
	13	.382	2.153
	14	.382	2.151
	15	.381	2.148
	16	.381	2.145
a	17	.380	2.143
a	18	.380	2.140
a	19	.380	2.138
a	20	.380	2.136

Size of aperture, 0.5 feet diameter, round.
Centre of aperture above weir, 1.806 feet.
Length of measuring weir, 2 feet.
Velocity of approach, 0.02 feet per second.
Head due to velocity, 0.0000 feet.
Temperature of water, 72° Fahr.
Height of water on weir for leakage, 0.07 feet.

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.......	7.65	—.28	7.37
a....	7.64	—.28	7.36

	Weir gauge.	Gauge No. 1. Centre of aperture.	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean...	.3817	2.1516	90.83	—.27	90.56	.3809	2.1516
a....	.3800	2.1392	90.04		90.04	.3800	2.1392

Experiment No. 53.

	Time. H. M.	Weir gauge.	Gauge No. 1. Centre of aperture.
b	1 35	.476	4.136
b	36	.476	4.141
b	37	.476	4.146
a	38	.477	4.159
a	39	.477	4.154
a	40	.478	4.157
a	41	.477	4.161
a	42	.477	4.165
a	43	.4775	4.168
c	44	.478	4.171

Size of aperture, 0.05 feet diameter, round.
Centre of aperture above weir, 1.806 feet.
Length of measuring weir, 2 feet.
Velocity of approach, 0.02 feet per second.
Head due to velocity, 0.0000 feet.
Temperature of water, 73° Fahr.
Height of water on weir for leakage, 0.08 feet.

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.......	9.75	—.26	9.49
a....	9.75	—.26	9.49
b....	9.73	—.26	9.47
c....	9.78	—.26	9.42

	Weir gauge.	Gauge No. 1. Centre of aperture.	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean.........	.4769	4.1558	125.32	.17	125.49	.4773	4.1558
a....	.4772	4.1607	125.44		125.44	.4772	4.1607
b....	.4760	4.1410	124.98		124.98	.4760	4.1410
c....	.4780	4.1710	125.74		125.74	.4780	4.1710

Experiment No. 54.

Size of aperture, 0.5 feet diameter, round.
Centre of aperture above weir, 1.806 feet.
Length of measuring weir, 4 feet.
Velocity of approach, 0.03 feet per second.
Head due to velocity, 0.0000 feet.
Temperature of water, 73° Fahr.
Height of water on weir for leakage, 0.10 feet.

Time.		Weir gauge.	Gauge No. 2. .02 below centre.
	H. M.		
a	2 24	.339	6.210
a	25	.338	6.250
a	26	.339	6.280
	27	.340	6.310
	28	.341	6.340
	29	.341	6.370
	30	.342	6.400
	31	.342	6.427
	32	.343	6.457
b	33	.344	6.485
b	34	.344	6.515

LEAKAGE, CUBIC FEET PER MINUTE.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.......	12.98	—.13	12.85
a....	12.79	—.13	12.66
b....	13.16	—.13	13.03

	Weir gauge.	Gauge No. 2.	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
Mean.........	.3412	6.3676	156.57	.38	156.95	.3418	6.3476
a....	.3387	6.2467	154.87		154.87	.3387	6.2267
b....	.3440	6.4850	158.47		158.47	.3440	6.4650

Experiment No. 55 A.

Size of aperture, 0.5 feet diameter, round.
Centre of aperture above weir, 1.806 feet.
Length of measuring weir, 4 feet.
Velocity of approach, *a, b,* 0.03 feet per second.
" " *c, d, e, f,* 0.04 "
" " *g, h, i, j,* 0.05 "
Head due to velocity, 0.0000 feet.
Temperature of water, 73° Fahr.

a	2 52	.357	7.275
a	53	.358	7.320
a	54	.358	7.365
b	3 5	.368	7.950
b	6	.369	8.030
b	7	.370	8.110
c	15	.383	8.910
c	16	.385	9.080
c	17	.387	9.220
d	27	.405	10.435
d	28	.407	10.535
d	29	.408	10.640
e	41	.425	11.890
e	42	.427	11.990
e	43	.429	12.120
f	49	.439	12.870
f	50	.440	13.000
f	51	.442	13.045

LEAKAGE, CUBIC FEET PER MINUTE.

	Weir height.	Observed leakage.	Correct'n for diff. of weir.	True leakage.
a....	.10	14.91	—.14	14.77
b....	.11	15.96	—.12	15.84
c....	.12	17.69	—.11	17.58
d....	.13	20.18	—.13	20.05
e....	.14	22.19	—.13	22.06
f....	.15	24.80	—.12	24.68
g....	.16	27.82	—.10	27.72
h....	.17	30.75	—.11	30.64
i....	.18	31.76	—.11	31.65
j....	.20	37.12	—.11	37.01

Experiment No. 55 A.—(*Continued.*)

Time.		Weir gauge.	Gauge No. 2. .02 below centre.	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on centre of aperture.
	H. M.							
g	4 1	.458	14.410					
	2	.460	14.490					
	3	.460	14.570					
h	13	.472	15.400					
	14	.473	15.480					
	15	.473	15.550					
i	19	.478	15.820					
	20	.479	15.870					
	21	.480	15.920					
j	24	.494	17.280					
	25	.494	17.285					
	26	.494	17.285					
	27	.495	17.285					
	28	.495	17.290					
	a	.358	7.320	168.12		168.12	.3580	7.300
	b	.369	8.030	175.84	.76	176.60	.3701	8.010
	c.....	.385	9.080	187.24	1.52	188.76	.3871	9.060
	d	.407	10.535	203.29	1.14	204.43	.4085	10.515
	e.....	.427	11.990	218.23	1.52	219.75	.4290	11.970
	f.....	.440	13.000	288.13	1.14	229.27	.4415	12.980
	g	.460	14.490	243.60		243.60	.4600	14.470
	h	.473	15.480	253.83		253.83	.4730	15.460
	i.....	.479	15.870	258.60	.76	259.36	.4800	15.850
	j.. ..	.4944	17.285	270.95	.15	271.10	.4946	17.265

Experiment No. 55 B.

Time.	Weir gauge.	Gauge No. 1.
H. M.		
5 56	.624	1.408
57	.623	1.404
58	.622	1.406
59	.6205	1.405
6 0	.620	1.411
1	.619	1.413

Size of aperture, 1.0007 feet diameter, round.
Aperture submerged.
Zero of gauge above weir, 1.806 feet.
Length of measuring weir, 4 feet.
Temperature of water, 72° Fahr.
Height of water on weir for leakage, 0.06 feet.
This aperture was in the bottom of the flume with its top surface 0.457 feet above the weir.

Experiment No. 55 B.—(*Continued.*)

Time. H. M.	Weir gauge.	Gauge No. 1.
a 6 2	.618	1.416
a 3	.619	1.419
a 4	.6185	1.421
a 5	.618	1.423
a 6	.618	1.425

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean..........	6.71	—.46	6.25
a.....	6.72	—.46	6.26

	Weir gauge.	Gauge No. 1.	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on aperture.
Mean..........	.6200	1.4141	378.07	—.46	377.61	.6195	2.6001
a.....	.6183	1.4208	376.54		376.54	.6183	2.6085

Experiment No. 56.

Size of aperture, 1.0007 feet diameter, round.
Aperture submerged.
Zero of gauge above weir, 1.786 feet.
Length of measuring weir, 4 feet.
Temperature of water, 72° Fahr.
Height of water on weir for leakage, 0.09 feet.
This aperture was in the bottom of the flume with its top surface, 0.457 feet above the weir.

Time. H. M.	Weir gauge.	Gauge No. 2.
6 26	.830	5.210
27	.832	5.250
28	.833	5.290
29	.835	5.330
30	.837	5.370
31	.840	5.405
32	.840	5.440
33	.842	5.465
34	.843	5.495
35	.843	5.520
36	.845	5.540
37	.845	5.560
38	.846	5.580
a 39	.848	5.600
a 40	.848	5.620
a 41	.847	5.640
a 42	.849	5.650

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean..........	11.29	—.40	10.89
a.....	11.60	—.40	11.20

	Weir gauge.	Gauge No. 2.	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on aperture.
Mean..........	.8414	5.4685	590.87	.90	591.77	.8423	6.4131
a.....	.8480	5.6275	597.62	.25	597.87	.8482	6.5655

Experiment No. 57.

Time.	Weir gauge.	Gauge No. 1.
H. M. 7 23	.745	3.626
24	.745	3.630
25	.745	3.634
26	.745	3.639
27	.744	3.645
28	.743	3.649
29	.743	3.653
30	.743	3.657
a 31	.744	3.662
a 32	.744	3.664
a 33	.744	3.666
Mean.........	.7441	3.6477
a.....	.7440	3.6640

Size of aperture, 1.0007 feet diameter, round.
Aperture submerged.
Zero of gauge above weir, 1.806 feet.
Length of measuring weir, 4 feet.
Temperature of water, 71° Fahr.
Height of water on weir for leakage, 0.08 feet.
This aperture was in the bottom of the flume with its top surface 0.457 feet above the weir.

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.........	9.22	—.43	8.79
a.....	9.23	—.43	8.80

	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on aperture.
Mean.........	493.90	—.08	493.82	.7440	4.7096
a.....	493.80		493.80	.7440	4.7260

Experiment No. 58 A.

Time.	Weir gauge.	Gauge No. 2.
H. M. 7 56	.899	6.970
57	.900	7.030
58	.902	7.080
59	.904	7.130
8 0	.906	7.175
1	.907	7.220
2	.909	7.265
3	.911	7.300
a 4	.912	7.335
a 5	.912	7.365
a 6	.913	7.390
a 7	.914	7.420
Mean.........	.9074	7.2230
a.....	.9127	7.3775

Size of aperture, 1.0007 feet diameter, round.
Aperture submerged.
Zero of gauge above weir, 1.786 feet.
Length of measuring weir, 4 feet.
Temperature of water, 71° Fahr.
Height of water on weir for leakage, 0.10 feet.
This aperture was in the bottom of the flume with its top surface 0.457 feet above the weir.

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.........	14.75	—.44	14.31
a.....	15.01	—.44	14.57

	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on aperture.
Mean.........	659.45	1.03	660.48	.9084	8.1019
a.....	665.12	.51	665.63	.9132	8.2508

Experiment No. 58 B.

Time.	Weir gauge.	Gauge No. 2.
H. M. 9 17	.495	7.510
18	.495	7.510
19	.495	7.505
20	.495	7.507
21	.495	7.507
22	.495	7.505
Mean.........	.4950	7.5073

Size of aperture, 1.0007 feet diameter, round. Aperture submerged. Zero of gauge above weir, 1.786 feet. Length of measuring weir, 10 feet. Temperature of water, 71° Fahr. Height of water on weir for leakage, 0.11 feet. This aperture was in the bottom of the flume with its top surface 0.457 feet above the weir.

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.........	15.20	—.19	15.01

Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on aperture.
688.94		688.94	.4950	8.7983

Experiment No. 59.

	Time.	Weir gauge.	Gauge No. 2.
	8 40	.549	10.690
	41	.550	10.735
	42	.551	10.775
a	43	.5515	10.810
a	44	.552	10.840
a	45	.552	10.870
a	46	.5525	10.895
	47	.553	10.915
	48	.553	10.935
	49	.554	10.950
	50	.554	10.965
Mean.........		.5520	10.8527
a		.5520	10.8537

Size of aperture, 1.0007 feet diameter, round. Aperture submerged. Zero of gauge above weir, 1.786 feet. Length of measuring weir, 10 feet. Temperature of water, 71° Fahr. Height of water on weir for leakage. 0.13 feet. This aperture was in the bottom of the flume with its top surface 0.457 feet above the weir.

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.........	20.76	—.19	20.57
a	20.76	—.19	20.57

	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on aperture.
Mean	810.36	.38	810.74	.5522	12.0867
a	810.36	.25	810.61	.5521	12.0877

Experiment No. 60.

Time.	Weir gauge.	Gauge No. 2.
9 8	.585	13.010
9	.585	13.025
10	.585	13.037
11	.5855	13.048
12	.5855	13.057
13	.585	13.063
14	.585	13.068
15	.585	13.072
Mean.........	.5851	13.0475

Size of aperture, 1.0007 feet diameter, round. Aperture submerged. Zero of gauge above weir, 1.786 feet. Length of measuring weir, 10 feet. Temperature of water, 71° Fahr. Height of water on weir for leakage, 0.15 feet. This aperture was in the bottom of the flume with its top surface 0.457 feet above the weir.

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.........	24.87	—.17	24.70

Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on aperture.
883.74		883.74	.5851	14.2484

Experiment No. 61.

Time.	Weir gauge.	Gauge No. 2.
H. M. 9 31	.613	15.090
32	.6135	15.097
33	.614	15.103
34	.615	15.110
35	.614	15.115
36	.614	15.120
a 37	.614	15.125
38	.614	15.125
39	.615	15.125
40	.614	15.127

Size of aperture, 1.0007 feet diameter, round.
Aperture submerged.
Zero of gauge above weir, 1.786 feet.
Length of measuring weir, 10 feet.
Temperature of water, 71° Fahr.
Height of water on weir for leakage, 0.17 feet.
This aperture was in the bottom of the flume with its top surface 0.457 feet above the weir.

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.......	29.75	—.18	29.57
a	29.77	—.18	29.59

	Weir gauge.	Gauge No. 2.	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on aperture.
Mean.........	.6140	15.1137	949.47	.08	949.55	.6140	16.2851
a	.6142	15.1245	949.93		949.93	.6142	16.2963

Experiment No. 62.

Time.	Weir gauge.	Gauge No. 2.
9 50	.644	17.520
51	.644	17.515
52	.644	17.522
53	.643	17.525
54	.644	17.530
55	.644	17.523
56	.644	17.520
57	.643	17.520
58	.643	17.525

Size of aperture, 1.0007 feet diameter, round.
Aperture submerged.
Zero of gauge above weir, 1.786 feet.
Length of measuring weir, 10 feet.
Temperature of water, 71° Fahr.
Height of water on weir for leakage, 0.21 feet.
This aperture was in the bottom of the flume with its top surface 0.457 feet above the weir.

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.......	38.60	—.15	38.45

	Weir gauge.	Gauge No. 2.	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on aperture.
Mean.........	.6438	17.5222	1018.81		1018.81	.6438	18.6644

Experiment No. 63.

Time.	Weir gauge.	Gauge No. 1.
11 6	.367	.901
7	.368	.904
8	.3685	.900

Size of aperture, 1.0000833 feet square.
Aperture submerged.
Zero of gauge above weir, 1.806 feet.
Length of measuring weir, 10 feet.
Temperature of water, 71° Fahr.
Height of water on weir for leakage, 0.06 feet.
This aperture was in the bottom of the flume with its top surface 0.441 feet above the weir.

EXPERIMENT No. 63.—(*Continued.*)

	Time.	Weir gauge.	Gauge No. 1.
	H. M.		
b	11 9	.370	.895
b	10	.370	.890
b	11	.370	.883
a	12	.3695	.880
a	13	.3695	.875
a	14	.3695	.870
	15	.369	.866
Mean.........		.3691	.8864
a....		.3695	.8750
b....		.3700	.8893

LEAKAGE, CUBIC FEET PER MINUTE.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.......	5.97	—.29	5.68
a....	5.96	—.29	5.67
b....	5.97	—.29	5.68

	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on aperture.
Mean	444.73	.17	444.90	.3692	2.3234
a	445.45		445.45	.3695	2.3120
b	446.35		446.35	.3700	2.3253

EXPERIMENT No. 64.

	Time.	Weir gauge.	Gauge No. 1.
	11 33	.437	2.439
	34	.438	2.459
	35	.439	2.485
	36	.440	2.513
	37	.441	2.540
	38	.441	2.568
	39	.442	2.583
a	40	.444	2.599
a	41	.444	2.616
a	42	.444	2.632
a	43	.444	2.647
Mean.........		.4413	2.5528
a....		.4440	2.6235

Size of aperture, 1.0000833 feet square.
Aperture submerged.
Zero of gauge above weir, 1.806 feet.
Length of measuring weir, 10 feet.
Temperature of water, 71° Fahr.
Height of water on weir for leakage, 0.07 feet.
This aperture was in the bottom of the flume with its top surface 0.441 feet above the weir.

LEAKAGE, CUBIC FEET PER MINUTE.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.......	8.11	—.26	7.85
a....	8.19	—.26	7.93

	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on aperture.
Mean	580.56	.53	581.09	.4416	3.9178
a	585.86		585.86	.4440	3.9855

EXPERIMENT No. 65.

Time.	Weir gauge.	Gauge No. 2.
H. M.		
12 1	.558	6.515
2	.559	6.580
3	.560	6.645
4	.562	6.700
5	.564	6.750

Size of aperture, 1.0000833 feet square.
Aperture submerged.
Zero of gauge above weir, 1.786 feet.
Length of measuring weir, 10 feet.
Temperature of water, 71° Fahr.
Height of water on weir for leakage, 0.10 feet.
This aperture was in the bottom of the flume with its top surface 0.441 feet above the weir.

EXPERIMENT No. 65.—(*Continued.*)

Time.		Weir gauge.	Gauge No. 2.
	H. M.		
a	12 6	.566	6.795
	7	.566	6.830
	8	.5665	6.863
	9	.567	6.893
	10	.568	6.920
	11	.568	6.940
Mean.........		.5640	6.7665
a.....		.5669	6.8735

LEAKAGE, CUBIC FEET PER MINUTE.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.........	13.69	—.25	13.44
a.....	13.97	—.25	13.72

	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on aperture.
Mean.........	836.73	.76	837.49	.5643	7.9885
a.....	843.14	.30	843.44	.5669	8.0926

EXPERIMENT No. 66.

Size of aperture, 1.0000833 feet square. Aperture submerged. Zero of gauge above weir, 1.786 feet. Length of measuring weir, 10 feet. Temperature of water, 72° Fahr. Height of water on weir for leakage, 0.13 feet. This aperture was in the bottom of the flume with its top surface 0.441 feet above the weir.

Time.	Weir gauge.	Gauge No. 2.
1 14	.640	10.430
15	.640	10.430
16	.640	10.430
17	.6405	10.430
18	.640	10.430
19	.640	10.432
20	.640	10.430
21	.640	10.430
Mean.........	.6401	10.4302

LEAKAGE, CUBIC FEET PER MINUTE.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.........	20.00	—.23	19.77

	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on aperture.
Mean.........	1009.88		1009.88	10.4302	11.5761

EXPERIMENT No. 67.

Size of aperture, 1.0000833 feet square. Aperture submerged. Zero of gauge above weir, 1.786 feet. Length of measuring weir, 10 feet. Temperature of water, 72° Fahr. Height of water on weir for leakage, 0.15 feet. This aperture was in the bottom of the flume with its top surface 0.441 feet above the weir.

Time.	Weir gauge.	Gauge No. 2.
1 41	.6885	13.180
42	.689	13.190
43	.689	13.197
44	.689	13.202
45	.689	13.210
46	.688	13.215
47	.689	13.220
48	.690	13.223
49	.689	13.222
50	.690	13.223
Mean.........	.6890	13.2082

LEAKAGE, CUBIC FEET PER MINUTE.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.........	25.08	—.21	24.87

	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on aperture.
Mean.........	1136.93	.17	1137.10	.6891	14.3052

Experiment No. 68.

Time.	Weir gauge.	Gauge No. 2.
H. M.		
2 2	.719	15.155
3	.721	15.155
4	.720	15.155
5	.720	15.157
6	.719	15.160
7	.720	15.155
8	.721	15.155
9	.721	15.152
10	.721	15.153
11	.7215	15.153
Mean.........	.7203	15.1550

Size of aperture, 1.0000833 feet square.
Aperture submerged.
Zero of gauge above weir, 1.786 feet.
Length of measuring weir, 10 feet.
Temperature of water, 72° Fahr.
Height of water on weir for leakage, 0.17 feet.
This aperture was in the bottom of the flume with its top surface 0.441 feet above the weir.

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.........	29.85	—.20	29.65

	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on aperture.
Mean.........	1203.82	.17	1203.99	.7204	16.2207

Experiment No. 69.

	Time.	Weir gauge.	Gauge No. 2.
b	2 18	.753	17.427
b	19	.754	17.423
b	20	.753	17.423
b	21	.754	17.425
b	22	.755	17.427
	23	.750	17.350
a	24	.755	17.440
a	25	.755	17.445
a	26	.754	17.445
Mean.........		.7537	17.4217
a.....		.7547	17.4433
b....		.7538	17.4230

Size of aperture, 1.0000833 feet square. Aperture submerged. Zero of gauge above weir, 1.786 feet. Length of measuring weir, 10 feet. Temperature of water, 72° Fahr. Height of water on weir for leakage, 0.19 feet. This aperture was in the bottom of the flume with its top surface, 0.441 feet above the weir.

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.........	37.94	—.21	37.73
a.....	38.13	—.21	37.92
b....	37.94	—.21	37.73

	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on aperture.
Mean.........	1287.65	.09	1287.74	.7537	18.4540
a.....	1290.19	—.38	1289.81	.7546	18.4746
b....	1287.90	.38	1288.28	.7539	18.4552

Experiment No. 70.

Time.	Weir gauge.	Gauge No. 1.
H. M.		
3 8	.546	1.712
9	.544	1.737
10	.548	1.760
11	.5485	1.778

Size of aperture, 1.0000833 feet square.
Aperture submerged with curved approach.
Zero of gauge above weir, 1.806 feet.
Length of measuring weir, 10 feet.
Temperature of water, 72° Fahr.
Height of water on weir for leakage, 0.07 feet.
Top of plate above weir, 0.441 feet.

EXPERIMENT No. 70.—(*Continued.*)

	Time.	Weir gauge.	Gauge No. 1.
	H. M. 3 12	.550	1.793
	13	.551	1.802
	14	.551	1.810
a	15	.552	1.818
a	16	.552	1.820
a	17	.552	1.820

LEAKAGE. CUBIC FEET PER MINUTE.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.........	7.20	—.40	6.80
a	7.24	—.40	6.84

	Weir gauge.	Gauge No. 1.	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on aperture.
Mean.........	.5494	1.7850	804.69	.51	805.20	.5496	3.0416
a	.5520	1.8193	810.36		810.36	.5520	3.0733

EXPERIMENT No. 71.

Size of aperture, 1.0000833 feet square.
Aperture submerged, with curved approach.
Zero of gauge above weir, 1.806 feet.
Length of measuring weir, 10 feet.
Temperature of water, 72° Fahr.
Height of water on weir for leakage, 0.08 feet.
Top of plate above weir, 0.441 feet.

	Time.	Weir gauge.	Gauge No. 1.
	3 43	.677	4.600
	44	.678	4.610
	45	.677	4.619
	46	.678	4.624
b	47	.6785	4.629
b	48	.679	4.636
b	49	.679	4.643
a	50	.680	4.648
a	51	.678	4.652
a	52	.680	4.657

LEAKAGE, CUBIC FEET PER MINUTE.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean........	10.24	—.37	9.87
a	10.26	—.37	9.89
b	10.25	—.37	9.88

	Weir gauge.	Gauge No. 1.	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on aperture,
Mean.........	.6785	4.6318	1101.50	.25	1101.75	.6786	5.7593
a	.6793	4.6523	1103.43		1103.43	.6793	5.7790
b	.6790	4.6360	1102.71	.19	1102.90	.6791	5.7630

EXPERIMENT No 72.

Size of aperture, 1.0000833 feet square.
Aperture submerged, with curved approach.
Zero of gauge above weir, 1.786 feet.
Length of measuring weir, 10 feet.
Temperature of water, 72° Fahr.
Height of water on weir for leakage, 0.12 feet.
Top of plate above weir, 0.441 feet.

Time.	Weir gauge.	Gauge No. 2.
H. M. 4 9	.828	9.445
10	.8285	9.495
11	.829	9.535
12	.831	9.565
13	.8315	9.593
14	.832	9.615

Experiment No. 72.—(*Continued.*)

	Time.	Weir gauge.	Gauge No. 2.
	H. M.		
a	4 15	.834	9.630
a	16	.835	9.647
a	17	.833	9.655
a	18	.835	9.675
Mean.........		.8317	9.5855
a....		.8342	9.6517

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.......	18.53	—.31	18.22
a....	18.64	—.31	18.33

	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on aperture.
Mean.........	1490.25	.57	1490.82	.8319	10.5398
a....	1496.90	.25	1497.15	.8343	10.6035

Experiment No. 73.

Size of aperture, 1.0000833 feet square. Aperture submerged, with curved approach. Zero of gauge above weir, 1.786 feet. Length of measuring weir, 10 feet. Temperature of water, 72° Fahr. Height of water on weir for leakage, 0.15 feet. Top of plate above weir, 0.441 feet.

Time.	Weir gauge.	Gauge No. 2.
4 35	.908	12.680
36	.9075	12.685
37	.908	12.690
38	.908	12.690
39	.906	12.693
40	.908	12.695
41	.908	12.695
42	.908	12.700
43	.908	12.705
Mean.........	.9077	12.6926

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.......	24.20	—.29	23.91

	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on aperture.
Mean.........	1696.50		1696.50	.9077	13.5709

Experiment No. 74.

Size of aperture, 1.0000833 feet square. Aperture submerged, with curved approach. Zero of gauge above weir, 1.786 feet. Length of measuring weir, 10 feet. Temperature of water, 72° Fahr. Height of water on weir for leakage, 0.21 feet. Top of plate above weir, 0.441 feet.

	Time.	Weir gauge.	Gauge No. 2.
	4 55	1.007	17.485
	56	1.008	17.485
a	57	1.006	17.480
a	58	1.005	17.480
a	59	1.008	17.410
a	5 0	1.006	17.460
a	1	1.008	17.430
a	2	1.008	17.430
a	3	1.005	17.420
a	4	1.005	17.415
a	5	1.006	17.420
Mean.........		1.0065	17.4468
a....		1.0063	17.4383

Leakage, Cubic Feet per Minute.

	Observed leakage.	Correction for diff. of weir.	True leakage.
Mean.......	38.15	—.30	37.85
a....	38.12	—.30	37.82

	Discharge cu. ft. per minute.	Correct'n for diff. of level.	Corrected discharge.	Corrected weir height.	Head on aperture.
Mean.........	1976.89		1976.89	1.0065	18.2263
a....	1976.31		1976.31	1.0063	18.2180

Table of Discharges with Apertures partly filled (as Weirs).

Aperture 2 Feet horizontal by 1.99975 Feet vertical. Area, 3.9995 Square Feet. Log. = 0.6020057.

	Number of experiments.	Length of weir in feet.	Mean depth on weir in feet.	Correction for difference in basin.	Corrected depth $= H$.	Velocity of approach, feet per second.	Head due to velocity $= h$.	Discharge, cubic feet per minute, $H + h$.	Discharge, cubic feet per minute, h.	Difference.	Leakage, cubic feet per minute.	True discharge, cubic feet per minute.	Head on bottom of aperture.
No. 1, mean	13	10	.6087	—.0002	.6085	.16	.0004	937.76	.02	937.74	7.58	930.16	1.8372
a	3	10	.6070	....	.6070	.16	.0004	934.33	.02	934.31	7.56	926.75	1.8290
b	2	10	.6100	....	.6100	.16	.0004	941.20	.02	941.18	7.59	933.59	1.8440
c	9	10	.6072	....	.6072	.16	.0004	934.78	.02	934.76	7.56	927.20	1.8326

Aperture 2 Feet horizontal by 1 Foot vertical. Area, 2 Square Feet. Log. = 0.3010300.

	Number of experiments.	Length of weir in feet.	Mean depth on weir in feet.	Correction for difference in basin.	Corrected depth $= H$.	Velocity of approach, feet per second.	Head due to velocity $= h$.	Discharge, cubic feet per minute, $H + h$.	Discharge, cubic feet per minute, h.	Difference.	Leakage, cubic feet per minute.	True discharge, cubic feet per minute.	Head on bottom of aperture.
No. 5, mean	12	6	.2847	.0002	.2849	.03	....	180.52	....	180.52	3.82	176.70	.5793
No. 6, mean	12	6	.3777	.0001	.3778	.05	....	274.85	....	274.85	3.97	270.88	.7849
No. 7, mean	11	6	.4800	....	.4800	.07	.0001	392.41	....	392.41	4.21	388.20	1.0079

Aperture 1.0000833 Feet Square. Area, 1.00017 Square Feet. Log. = 0.0000738.

	Number of experiments.	Length of weir in feet.	Mean depth on weir in feet.	Correction for difference in basin.	Corrected depth $= H$.	Velocity of approach, feet per second.	Head due to velocity $= h$.	Discharge, cubic feet per minute, $H + h$.	Discharge, cubic feet per minute, h.	Difference.	Leakage, cubic feet per minute.	True discharge, cubic feet per minute.	Head on bottom of aperture.
No. 32, mean	8	4	.3930	....	.3930	.03	....	193.12	....	193.12	9.56	183.56	1.0160

Table of Discharges and Coefficients with Head above the Top of Aperture.

Aperture 2 Feet horizontal by 1.99975 Feet vertical. Area, 3.9995 Square Feet. Log. = 0.6020057.

	Number of experiments.	Length of weir in feet.	Mean depth on weir in feet.	Correction for difference in basin.	Corrected depth $= H$.	Velocity of approach, feet per second.	Head due to velocity $= h$.	Discharge, cubic feet per minute, $H + h$.	Discharge, cubic feet per minute, h.	Difference.	Leakage, cubic feet per minute.	True discharge, cubic feet per minute.	Mean head on centre of aperture.	Coefficient.
No. 2, mean........	20	10	.9055	.0001	.9056	.28	.0012	1694.01	.08	1693.93	10.05	1683.88	2.0660	.60871
a........	4	10	.9090	....	.9090	.28	.0012	1703.42	.08	1703.34	10.05	1693.29	2.0720	.61122
No. 3, mean........	17	10	1.0196	....	1.0196	.33	.0017	2020.02	.14	2019.88	11.39	2008.49	3.0469	.59787
a........	4	10	1.0172	....	1.0172	.33	.0017	2013.04	.14	2012.90	11.37	2001.53	3.0370	.59676
b........	5	10	1.0204	....	1.0204	.33	.0017	2022.35	.14	2022.21	11.41	2010.80	3.0604	.59723
c........	6	10	1.0196	—.0001	1.0195	.33	.0017	2019.73	.14	2019.59	11.39	2008.20	3.0510	.59737
No. 4, mean........	16	10	1.0819	.0002	1.0821	.36	.0020	2206.37	.18	2206.19	11.89	2194.30	3.5408	.60591
a........	8	10	1.0855	....	1.0855	.36	.0020	2216.60	.18	2216.42	11.89	2204.53	3.5436	.60849
b........	8	10	1.0782	.0004	1.0786	.36	.0020	2195.86	.18	2195.68	11.89	2183.79	3.5379	.60325

Aperture 2 Feet horizontal by 1 Foot vertical. Area, 2 Square Feet. Log. = 0.3010300.

	Number of experiments.	Length of weir in feet.	Mean depth on weir in feet.	Correction for difference in basin.	Corrected depth $= H$.	Velocity of approach, feet per second.	Head due to velocity $= h$.	Discharge, cubic feet per minute, $H + h$.	Discharge, cubic feet per minute, h.	Difference.	Leakage, cubic feet per minute.	True discharge, cubic feet per minute.	Mean head on centre of aperture.	Coefficient.
No. 8, mean........	26	6	.7627	.0002	.7629	.13	.0003	778.95	.01	778.94	5.30	773.64	1.8096	.59757
a........	7	6	.7653	.0001	.7654	.13	.0003	782.72	.01	782.71	5.31	777.40	1.8296	.59718
b........	5	6	.7608	.0001	.7609	.13	.0003	775.95	.01	775.94	5.28	770.66	1.7962	.59748
c........	5	6	.7632	....	.7632	.13	.0003	779.40	.01	779.39	5.30	774.09	1.8118	.59755
d........	3	6	.7640	....	.7640	.13	.0003	780.61	.01	780.60	5.30	775.30	1.8200	.59714

No. 9, mean........	18	6	.9100	.0001	.9101	.17	.0004	1009.90	.01	1009.89	5.91	1003.98	3.0328	.59902
a........	6	6	9118	....	.9118	.17	.0004	1012.68	.01	1012.67	5.92	1006.75	3.0512	.59886
b........	3	6	.9110	....	.9110	.17	.0004	1011.37	.01	1011.36	5.91	1005.45	3.0403	.59916
c........	8	6	.9086	....	.9086	.17	.0004	1007.46	.01	1007.45	5.90	1001.55	3.0204	.59880
No. 10 A, mean.....	18	6	1.0526	.0001	1.0527	.20	.0006	1250.40	.02	1250.38	7.39	1242.99	4.6614	.59820
a........	8	6	1.0537	....	1.0537	.20	.0006	1252.14	.02	1252.12	7.41	1244.71	4.6764	.59807
b........	6	6	1.0520	.0001	1.0521	.20	.0006	1249.36	.02	1249.34	7.37	1241.97	4.6540	.59819
No. 10 B, c.........	6	10	.7398	....	.7398	.22	.0008	1254.55	.05	1254.50	7.55	1246.95	4.6890	.59834
No. 11, mean.......	18	10	.7873	.0001	.7874	.23	.0008	1376.10	.05	1376.05	9.42	1366.63	5.6667	.59652
a........	3	10	.7900	....	.7900	.23	.0008	1382.83	.05	1382.78	9.47	1373.31	5.7100	.59716
b........	6	10	.7870	....	.7870	.23	.0008	1375.06	.05	1375.01	9.42	1365.59	5.6717	.59580
c........	3	10	.7890	.0001	.7891	.23	.0008	1380.50	.05	1380.45	9.46	1370.99	5.7000	.59672
d........	6	10	.7853	.0001	.7854	.23	.0008	1370.92	.05	1370.87	9.37	1361.50	5.6250	.59648
No. 12, mean.......	12	10	.8412	....	.8412	.26	.0011	1518.40	.07	1518.37	10.88	1507.49	6.8675	.59771
a........	3	10	.8420	....	.8420	.26	.0011	1520.64	.07	1520.57	10.88	1509.69	6.8650	.59870
No. 13, mean.......	11	10	.8741	....	.8741	.27	.0011	1607.26	.07	1607.19	12.11	1595.08	7.6864	.59781
No. 14, mean.......	10	10	.9049	....	.9049	.28	.0012	1692.07	.08	1691.99	13.20	1678.79	8.4761	.59915
No. 15, mean.......	11	10	.9465	.0001	.9466	.30	.0014	1809.23	.10	1809.13	15.24	1793.89	9.6464	.60014
a........	5	10	.9472	....	.9472	.30	.0014	1810.93	.10	1810.83	15.24	1795.59	9.6540	.60047
No. 16, mean.......	10	10	1.0051	....	1.0051	.33	.0017	1977.76	.14	1977.62	20.20	1957.42	11.3140	.60466
a........	6	10	1.0063	....	1.0063	.33	.0017	1981.26	.14	1981.12	20.20	1960.92	11.3150	.60572

APERTURE 2 FEET HORIZONTAL BY 0.5 FOOT VERTICAL. AREA, 1 SQUARE FOOT. LOG. = 0.0000000.

	Number of experiments.	Length of weir in feet.	Mean depth on weir in feet.	Correction for difference in basin.	Corrected depth = H.	Velocity of approach, feet per second.	Head due to velocity = h.	Discharge, cubic feet per minute, $H + h$.	Discharge, cubic feet per minute, h.	Difference.	Leakage, cubic feet per minute.	True discharge, cubic feet per minute.	Mean head on centre of aperture.	Coefficient.
No. 17, mean.......	9	5	.5096	.0001	.5095	.06	.0001	356.01	....	356.01	4.93	351.08	1.4239	.61141
a........	7	5	.5094	....	.5094	.06	.0001	355.91	....	355.91	4.93	350.98	1.4220	.61165
b........	4	5	.5092	....	.5092	.06	.0001	355.71	....	355.71	4.93	350.78	1.4230	.61108
No. 18, a........	7	5	.6490	....	.6490	.09	.0001	508.87	....	508.87	5.65	503.22	2.9303	.61090
b........	10	5	.6453	.0007	.6460	.09	.0001	505.41	....	505.41	5.60	499.81	2.8806	.61197
No. 19, mean.......	10	5	.7623	....	.7623	.11	.0002	644.87	....	644.87	7.31	637.56	4.7460	.60817
No. 20, a........	6	5	.8452	....	.8452	.13	.0003	750.40	.01	750.39	9.91	740.48	6.3848	.60899
b........	4	5	.8427	....	.8427	.13	.0003	747.14	.01	747.13	9.89	737.24	6.3605	.60748
c........	3	5	.8420	....	.8420	.13	.0003	746.23	.01	746.22	9.82	736.40	6.3227	.60860
No. 21, mean.......	14	5	.9323	.0001	.9324	.14	.0003	866.29	.01	866.28	12.91	853.37	8.5395	.60686
a........	7	5	.9320	....	.9320	.14	.0003	865.75	.01	865.74	12.91	852.83	8.5447	.60629
No. 22 A, mean.....	13	5	.9714	.0002	.9716	.15	.0003	919.97	.01	919.96	14.75	905.21	9.6302	.60618
a........	8	5	.9721	....	.9721	.15	.0003	920.67	.01	920.66	14.79	905.87	9.6480	.60607
No. 22 B, mean.....	12	5	1.0356	.0002	1.0358	.17	.0004	1010.04	.01	1010.03	20.22	989.81	11.5558	.60509
No. 22 C, mean.....	5	10	.6390	....	.6390	.18	.0005	1008.70	.02	1008.68	20.33	988.35	11.5728	.60334
No. 23 A, mean.....	12	10	.6770	....	.6770	.19	.0006	1099.34	.03	1099.31	30.76	1068.55	13.5065	.60422

No. 23 B, mean.....	11	10	.7051	.0001	.7052	.20	.0006	1168.01	.03	1167.98	43.72	1124.26	15.0565	.60211
a.........	5	10	.7054	..........•	.7054	.20	.0006	1168.50	.03	1168.47	43.72	1124.75	15.0598	.60231
No. 24, mean.......	12	10	.7373	—.0001	.7372	.21	.0007	1247.77	.04	1247.73	57.18	1190.55	16.9652	.60067
a........	8	10	.7367	....	.7367	.21	.0007	1246.51	.04	1246.47	57.18	1189.29	16.9657	.60063

APERTURE 2 FEET DIAMETER, ROUND. AREA, 3.14159 SQUARE FEET. LOG. = 0.4971499.

No. 25, mean.......	13	10	.7128	.0002	.7130	.21	.0007	1187.48	.04	1187.44	5.00	1182.44	1.7677	.58829
a........	5	10	.7148	.0001	.7149	.21	.0007	1192.17	.04	1192.13	5.01	1187.12	1.7760	.58924
No. 26, mean.......	13	10	.8159	....	.8159	.25	.0010	1451.09	.06	1451.03	5.39	1445.64	2.5958	.59353
a.........	7	10	.8166	....	.8166	.25	.0010	1452.93	.06	1452.87	5.39	1447.48	2.6030	.59346
No. 27, mean.......	14	10	.9904	.0001	.9905	.32	.0016	1935.19	.13	1935.06	6.75	1928.31	4.4735	.60308
a.........	9	10	.9903	....	.9903	.32	.0016	1934.61	.13	1934.48	6.76	1927.72	4.4849	.60283
No. 28, mean.......	13	10	1.0921	.0001	1.0922	.36	.0020	2236.82	.18	2236.64	8.91	2227.73	5.8335	.61012
a........	8	10	1.0906	....	1.0906	.36	.0020	2231.99	.18	2231.81	8.90	2222.91	5.8269	.60915
b........	4	10	1.0947	.0002	1.0949	.36	.0020	2244.98	.18	2244.80	8.95	2235.85	5.8550	.60794
No. 29, mean.......	12	10	1.1606	.0001	1.1607	.39	.0024	2447.93	.23	2447.70	10.24	2437.46	6.9333	.61163
a........	8	10	1.1614	....	1.1614	.39	.0024	2450.11	.23	2449.88	10.26	2439.62	6.9519	.61205
b.........	4	10	1.1600	....	1.1600	.39	.0024	2445.75	.23	2445.52	10.22	2435.30	6.9087	.61323
No. 30, mean.......	11	10	1.2357	....	1.2357	.43	.0029	2685.95	.31	2685.64	12.29	2673.35	8.3432	.61222
a........	6	10	1.2373	....	1.2373	.43	.0029	2691.07	.31	2690.76	12.32	2678.44	8.3608	.61274
No. 31, mean.......	8	10	1.3022	....	1.3022	.46	.0033	2902.49	.38	2902.11	14.35	2887.76	9.6381	.61530

APERTURE 1.0000833 FEET SQUARE. AREA, 1.00017 SQUARE FEET. LOG. = 0.0000738.

	Number of experiments.	Length of weir in feet.	Mean depth on weir in feet.	Correction for difference in basin.	Corrected depth $=H$.	Velocity of approach, feet per second.	Head due to velocity $=h$.	Discharge, cubic feet per minute, $H+h$.	Discharge, cubic feet per minute, h.	Difference.	Leakage, cubic feet per minute.	True discharge, cubic feet per minute.	Mean head on centre of aperture.	Coefficient.
No. 33, mean.......	13	4	.5942	.0002	.5944	.06	.0001	355.45	....	355.45	11.45	344.00	1.4892	.58590
a........	7	4	.5950	....	.5950	.06	.0001	355.98	....	355.98	11.45	344.53	1.4941	.58564
b........	5	4	.5930	....	.5930	.06	.0001	354.22	....	354.22	11.44	342.78	1.4796	.58230
No. 34, mean.......	18	4	.8172	.0003	.8175	.10	.0002	566.79	....	566.79	13.50	553.29	3.6902	.59844
a........	8	4	.8196	....	.8196	.10	.0002	568.91	....	568.91	13.52	555.39	3.7174	.59851
b........	3	4	.8170	....	.8170	.10	.0002	566.28	....	566.28	13.50	552.78	3.6887	.59801
No. 35, mean.......	19	4	.8937	.0002	.8939	.11	.0002	645.46	....	645.46	15.14	630.32	4.8036	.59755
a........	5	4	.8940	....	.8940	.11	.0002	645.56	....	645.56	15.18	630.28	4.8164	.60005
b........	10	4	.8930	....	.8930	.11	.0002	644.51	....	644.51	15.11	629.40	4.7896	.59755
No. 36, mean.......	11	4	.9335	....	.9335	.11	.0002	687.39	....	687.39	16.04	671.35	5.4908	.59529
No. 37, mean.......	20	4	1.0060	.0005	1.0065	.13	.0003	766.72	....	766.72	18.48	748.24	6.7211	.59967
a........	3	4	1.0100	.0003	1.0103	.13	.0003	770.58	....	770.58	18.70	751.88	6.8167	.59835
No. 38 A, mean.....	14	4	1.1436	.0004	1.1440	.15	.0003	922.31	....	922.31	23.98	898.73	9.8938	.59612
a........	3	4	1.1451	....	1.1451	.15	.0003	923.58	....	923.58	24.02	899.56	9.8293	.59616
b........	5	4	1.1412	....	1.1412	.15	.0003	919.06	....	919.06	23.94	895.12	9.7850	.59456
No. 38 B, mean.....	7	9.013	.6513	....	.6513	.16	.0004	935.06	.01	935.05	24.52	910.53	9.9001	.60127
d........	3	9.013	.6520	....	.6520	.16	.0004	936.55	.01	936.54	24.52	912.02	9.9047	.60211
No. 39, mean.......	10	10	.6472	....	.6472	.18	.0005	1028.00	.02	1027.98	27.93	1000.05	12.0046	.59971

No. 40, mean.......	10	10	.6768	—.0001	.6767	.19	.0006	1098.61	.03	1098.58	31.21	1067.37	13.6293	.60072
a	4	10	.6765	....	.6765	.19	.0006	1098.13	.03	1098.10	31.21	1066.89	13.6360	.60030
No. 41, mean.......	10	10	.7023	....	.7023	.20	.0006	1160.87	.03	1160.84	36.04	1124.80	15.1322	.60079
a	8	10	.7025	....	.7025	.20	.0006	1161.37	.03	1161.34	36.04	1125.30	15.1321	.60106
No. 42, mean.......	10	10	.7368	....	.7368	.21	.0007	1246.76	.04	1246.72	42.78	1203.94	17.5647	.59687
Aperture, 1.0007 Feet Diameter, round. Area, 0.78650 Square Feet. Log. = $\bar{1}$.8956977.														
No. 43, mean.......	10	4	.4605	—.0001	.4604	.04	....	243.91	....	243.91	11.05	232.86	1.1473	.57442
a	4	4	.4600	....	.4600	.04	....	243.60	....	243.60	11.05	232.55	1.1470	.57373
b	4	4	.4610	....	.4610	.04	....	244.38	....	244.38	11.05	233.33	1.1478	.57545
No. 44, mean.......	15	4	.5932	.0005	.5937	.06	.0001	354.83	....	354.83	12.58	342.25	2.3607	.58856
a	6	4	.5953	....	.5953	.06	.0001	356.24	....	356.24	12.61	343.63	2.3810	.58706
b	3	4	.5940	....	.5940	.06	.0001	355.10	....	355.10	12.60	342.50	2.3687	.58800
No. 45, mean.......	14	4	.7548	.0002	.7550	.09	.0001	504.60	....	504.60	14.80	489.80	4.8091	.59014
a	7	4	.7550	....	.7550	.09	.0001	504.60	....	504.60	14.82	489.78	4.8159	.58970
b	3	4	.7560	....	.7560	.09	.0001	505.57	....	505.57	14.87	490.70	4.8393	.58933
No. 46 A, mean.....	12	4	.8932	—.0005	.8927	.11	.0002	644.20	....	644.20	21.08	623.12	7.9705	.58813
a	4	4	.8969	....	.8969	.11	.0002	648.61	....	648.61	21.04	627.57	7.9520	.58802
No. 46 B, mean.....	8	10	.4752	....	.4752	.12	.0002	648.69	.01	648.68	21.32	627.36	7.9172	.58912
b	4	10	.4745	....	.4745	.12	.0002	647.27	.01	647.26	21.32	625.94	7.9172	58778
No. 47, mean.......	11	10	.5321	.0001	.5322	.14	.0003	768.11	.01	768.10	26.12	741.98	10.8819	.59431
a	5	10	.5330	....	.5330	.14	.0003	769.83	.01	769.82	26.16	743.66	10.9212	.59432
b	4	10	.5320	....	.5320	.14	.0003	767.68	.01	767.67	26.11	741.56	10.8687	.59433

APERTURE 1.0007 FEET DIAMETER, ROUND (*Continued*). AREA, 0.78650 SQUARE FEET. LOG. = 1.8956977.

	Number of experiments.	Length of weir in feet.	Mean depth on weir in feet.	Correction for difference in basin.	Corrected depth = H.	Velocity of approach, feet per second.	Head due to velocity = h.	Discharge, cubic feet per minute, $H+h$.	Discharge, cubic feet per minute, h.	Difference.	Leakage, cubic feet per minute.	True discharge, cubic feet per minute.	Mean head on centre of aperture.	Coefficient.
No. 48, mean......	11	10	.5574	.0001	.5575	.15	.0003	823.08	.01	823.07	28.83	794.24	12.4770	.59411
a........	5	10	.5582	....	.5582	.15	.0003	824.62	.01	824.61	28.86	795.75	12.5070	.59453
b........	4	10	.5570	....	.5570	.15	.0003	821.98	.01	821.97	28.81	793.16	12.4672	.59353
No. 49, mean......	10	10	.5827	....	.5827	.16	.0004	879.25	.02	879.23	32.45	846.78	14.1269	.59528
a........	5	10	.5828	....	.5828	.16	.0004	879.48	.02	879.46	32.47	846.99	14.1444	.59508
b........	5	10	.5821	....	.5821	.16	.0004	877.91	.02	877.89	32.41	845.48	14.1094	.59473
No. 50, mean......	12	10	.6052	....	.6052	.16	.0004	930.21	.02	930.19	38.01	892.18	15.6640	.59562
a........	6	10	.6057	....	.6057	.16	.0004	931.35	.02	931.33	38.01	893.32	15.6658	.59635
b........	5	10	.6047	....	.6047	.16	.0004	929.07	.02	929.05	38.01	891.04	15.6636	.59487
No. 51, mean......	10	10	.6345	....	.6345	.17	.0004	997.94	.02	997.92	43.16	954.76	17.7245	.60128
a........	4	10	.6352	....	.6352	.17	.0004	999.57	.02	999.55	43.21	956.34	17.7400	.59994
b........	5	10	.6340	....	.6340	.17	.0004	996.77	.02	996.75	43.14	953.61	17.7150	.59865

APERTURE 0.5 FEET DIAMETER, ROUND. AREA, 0.19635 SQUARE FEET. LOG. = $\bar{1}.2930299$.

	Number of experiments.	Length of weir in feet.	Mean depth on weir in feet.	Correction for difference in basin.	Corrected depth = H.	Velocity of approach, feet per second.	Head due to velocity = h.	Discharge, cubic feet per minute, $H+h$.	Discharge, cubic feet per minute, h.	Difference.	Leakage, cubic feet per minute.	True discharge, cubic feet per minute.	Mean head on centre of aperture.	Coefficient.
No. 52, mean......	12	2	.3817	—.0008	.3809	.02	....	90.56	....	....	7.37	83.19	2.1516	.60025
a........	4	2	.3800	....	.3800	.02	....	90.04	....	....	7.36	82.68	2.1392	.59829
No. 53, mean......	10	2	.4769	.0004	.4773	.02	....	125.49	....	....	9.49	116.00	4.1558	.60224

a........	6	2	.4772	....	.4772	.02	....	125.44	....	....	9.49	115.95	4.1607	.60163
b........	3	2	.4760	....	.4760	.02	....	124.98	....	....	9.47	115.51	4.1410	.60077
c........	1	2	.4780	....	.4780	.02	....	125.74	....	....	9.42	116.32	4.1710	.60280
No. 54, mean.......	11	4	.3412	.0006	.3418	.03	....	156.95	....	....	12.85	144.10	6.3476	.60534
a........	3	4	.3387	....	.3387	.03	....	154.87	....	....	12.66	142.21	6.2267	.60317
b........	1	4	.3440	....	.3440	.03	....	158.47	....	....	13.03	145.44	6.4650	.60539
No. 55 A, a........	1	4	.3580	....	.3580	.03	....	168.12	....	....	14.77	153.35	7.3000	.60076
b........	1	4	.3690	.0011	.3701	.03	....	176.60	....	....	15.84	160.76	8.0100	.60117
c........	1	4	.3850	.0021	.3871	.04	....	188.76	....	....	17.58	171.18	9.0600	.60191
d........	1	4	.4070	.0015	.4085	.04	....	204.43	....	....	20.05	184.38	10.5150	.60114
e........	1	4	.4270	.0020	.4290	.04	....	219.75	....	....	22.06	197.69	11.9700	.59996
f........	1	4	.4400	.0015	.4415	.04	....	229.27	....	....	24.68	204.59	12.9800	.60102
g........	1	4	.4600	....	.4600	.05	....	243.60	....	....	27.72	215.88	14.4700	.60064
h........	1	4	.4730	....	.4730	.05	....	253.83	....	....	30.64	223.19	15.4600	.60077
i........	1	4	.4790	.0010	.4800	.05	....	259.36	....	....	31.65	227.71	15.8500	.60535
j........	5	4	.4944	.0002	.4946	.05	....	271.10	....	....	37.01	234.09	17.2650	.59626

APERTURE 1.0007 FEET DIAMETER, SUBMERGED. AREA, 0.78650 SQUARE FEET. LOG. = $\overline{1}.8956977$.

No. 55 B, mean.....	11	4	.6200	—.0005	.6195	....	....	377.61	....	....	6.25	371.36	2.6001	.60851
a........	5	4	.6183	....	.6183	....	....	376.54	....	....	6.26	370.28	2.6185	.60577
No. 56, mean.......	17	4	.8414	.0009	.8423	....	....	591.77	....	....	10.89	580.83	6.4131	.60608
a........	4	4	.8480	.0002	.8482	....	....	597.87	....	....	11.20	586.67	6.5655	.60497
No. 57, mean.......	11	4	.7441	—.0001	.7440	....	....	493.80	....	....	8.79	485.01	4.7096	.59051

Aperture, 1.0007 Feet Diameter, submerged. (*Continued.*) Area, 0.78650 Square Feet. Log. = $\bar{1}.8956977$.

	Number of experiments.	Length of weir in feet.	Mean depth on weir in feet.	Correction for difference in basin.	Corrected depth $=H$.	Discharge, cubic feet per minute.	Leakage, cubic feet per minute.	True discharge cubic feet per minute.	Mean head on centre of aperture.	Coefficient.
No. 57, a	3	4	.7440	....	.7440	493.80	8.80	485.00	4.7260	.58948
No. 58 A, mean	12	4	.9074	.0010	.9084	660.48	14.31	646.17	8.1019	.59982
a	4	4	.9127	.0005	[illegible]	665.63	14.57	651.06	8.2508	.59888
No. 58 B, mean	6	10	.4950	....	.4950	688.94	15.01	673.93	8.7983	.60032
No. 59, mean	11	10	.5520	.0002	.5522	810.74	20.57	790.17	12.0867	.60054
a	4	10	.5520	.0001	.5521	810.61	20.57	790.04	12.0377	.60041
No. 60, mean	8	10	.5851	....	.5851	883.74	24.70	859.04	14.2484	.60131
No. 61, mean	10	10	.6140	....	.6140	949.55	29.57	919.98	16.2851	.60236
a	6	10	.6142	....	.6142	949.93	29.59	920.34	16.2963	.60239
No. 62, mean	9	10	.6438	....	.6438	1018.81	38.45	980.36	18.6644	.59926

Aperture, 1.0000833 Feet Square, submerged. Area, 1.00017 Square Feet. Log. = 0.0000738.

No. 63, mean	10	10	.3691	.0001	.3692	444.90	5.68	439.22	2.3234	.59871
a	3	10	.3695	....	.3695	445.45	5.67	439.78	2.3120	.60164
b	3	10	.3700	....	.3700	446.35	5.68	440.67	2.3253	.60044
No. 64, mean	11	10	.4413	.0003	.4416	581.09	7.85	573.24	3.9178	.60174

No. 64, a	4	10	.4440	....	.4440	585.86	7.93	577.93	3.9855	.60149
No. 65, mean	11	10	.5640	.0003	.5643	837.49	13.44	824.05	7.9885	.60578
a	6	10	.5669	....	.5669	843.44	13.72	829.72	8.0926	.60601
No. 66, mean	8	10	.6040	....	.6040	1009.88	19.77	990.11	11.5761	.60464
No. 67, mean	10	10	.6890	.0001	.6891	1137.10	24.87	1112.23	14.3052	.61100
No. 68, mean	10	10	.7203	.0001	.7204	1203.99	29.65	1174.34	16.2207	.60584
No. 69, mean	9	10	.7537	....	.7537	1287.74	37.73	1250.01	18.4540	.60459
a	3	10	.7547	−.0001	.7546	1289.81	37.92	1251.89	18.4716	.60417
b	5	10	.7538	.0001	.7539	1288.28	37.73	1250.55	18.4552	.60482

SAME APERTURE WITH CURVED APPROACH. AREA, 1.00017 SQUARE FEET. LOG. = 0.0000738.

No. 70, mean	10	10	.5494	.0002	.5496	805.20	6.80	798.40	3.0416	.95118
a	3	10	.5520	....	.5520	810.36	6.84	803.52	3.0733	.95233
No. 71, mean	10	10	.6785	.0001	.6786	1101.75	9.87	1091.88	5.7593	.94533
a	3	10	.6793	....	.6793	1103.43	9.89	1093.54	5.7790	.94516
b	3	10	.6790	.0001	.6791	1102.90	9.88	1093.02	5.7630	.94602
No. 72, mean	10	10	.8317	.0002	.8319	1490.82	18.22	1472.60	10.5398	.94246
a	4	10	.8342	.0001	.8343	1497.15	18.33	1478.82	10.6035	.94359
No. 73, mean	9	10	.9077	....	.9077	1696.50	23.91	1672.59	13.5709	.94337
No. 74, mean	11	10	1.0065	....	1.0065	1976.89	37.85	1939.04	18.2263	.94370
a	9	10	1.0063	....	1.0063	1976.31	37.82	1938.49	18.2180	.94364

DISCUSSION

ON TESTS AND TESTING MACHINES.

(Continued from Vol. IV, page 276.)

Mr. Thomas C. Clarke :—At the time of making the following experiments upon English steel wire, there was, so far as the writer knew, no publication of tests of its elasticity. To ascertain the effect of direct vertical tension, a tower of an unfinished building was used. The ends of the wire were carried over two hooks, 719 inches apart, coiled around itself, and firmly lashed with unannealed wire of about No. 20 B. w. g. It was found that 6 or 8 lashings were necessary to prevent the test wire from drawing out. The upper hook was fastened to a beam in the tower ; a scale pan weighing, with test wire and hooks, 198 pounds, was attached to lower end of the wire. An index, with needle point, was fixed to the lower hook, and the elongations read on a boxwood scale, divided to hundredths of an inch, and firmly fixed. Size of wire tested, No. 9 B. w. g., area of section, $0.0166 = \frac{1}{60}$ square inch; 18.33 feet in length, weighed one pound.

Weight. Pounds.	Elongation. Inches.	Weight. Pounds.	Elongation. Inches.	Weight. Pounds.	Elongation. Inches.	Weight. Pounds.	Elongation. Inches.	Weight. Pounds.	Elongation. Inches.
198	0. (*a*)	798	0.95 (*b*)	198	0.	748	0.85	1398	1.88
346	0.23	———	———	248	0.1	798	0.95	1498	2.08
374	0.29	798	0.85 (*c*)	298	0.2	848	1.	1598	2.28
411	0.32	848	0.9	348	0.25	898	1.1	1698	2.48
458	0.4	898	1.	398	0.35	948	1.15	1798	2.68(*g*)
492	0.45	948	1.1	448	0.4	998	1.2 (*f*)	———	———
588	0.52	998	1.2	498	0.5	———	———	1798	2.7
599	0.6	1048 (*d*)	1.3	548	0.55	998	1.2	2198	3.6
705	0.8	———	———	598	0.65	1098	1.3	2798	7.6
714	0.81	700	0.8 (*e*)	648	0.7	1198	1.55	2916 (*h*)	……..
748	0.85	———	———	698	0.8	1298	1.7	……..	……..

(*a*) Temperature, 80° Fahr. With the wire sustaining 198 pounds (scale pan, &c.) the index was set at zero, and additional weights applied. (*b*) When weights from the scale pan (600 pounds) were removed, the index returned to zero. (*c*) Thermometer had fallen to 70° and the wire contracted 0.05 inch, whence per inch the contraction was 0.0000695 and per inch per degree, 0.000 00695 inches. (*d*) The splice broke. (*e*) A new splice was made, leaving the wire 716 inches long, at temperature 68°. The weights were left on 4 days; when those from scale pan (502 pounds) were removed, the index returned to 0.08, at 74° temperature. A new zero was then taken. (*f*) Left on 1.5 hours; when weights in scale pan (800 pounds) were removed, the index returned to zero, at 74° temperature, thus showing no set at 48 000 pounds tension per square inch. (*g*) When weights in scale pan (800 pounds) were removed, the index returned to 1.55, at 74° temperature, whence permanent set was 0.35 at 96 000 pounds tension per square inch. (*h*) The wire broke slowly—tension 174 960 pounds per square inch.

AMERICAN SOCIETY OF CIVIL ENGINEERS.

INCORPORATED 1852.

TRANSACTIONS.

CXIX.

ERECTION OF THE VERRUGAS BRIDGE.

A Paper by L. LEFFERTS BUCK, C. E., Member of the Society.

READ DECEMBER 1ST, 1875.

In an article from the *Railroad Gazette*, September 11th, 1875, headed "Rapid Renewal of Bridges," appears an extract* giving inquiries made by Mr. J. Dutton Steele, regarding the plan of erection of the Verrugas Bridge. I was in charge of the work as engineer, from the laying of the first stone to completion. The plans followed in its erection were my original design. Articles have appeared in different papers at various times describing the work; but none that I have ever seen gave a very clear idea of the manner in which it was handled, or of the difficulties encountered. Hence, I present the following:

The Verrugas Bridge is situated on the Lima & Oroya R. R., in Peru, S. A., 51.8 miles from Callao. The mean elevation of its grade line above sea level is 5 836 feet. The ravine or *quebrada*, which it crosses, furnishes a channel for a very small stream or rill, which, coming from the mountains by a rapid descent, and flowing nearly northward, is discharged into the Rimac river, about 800 feet north of the bridge.

The earth in the vicinity of the bridge is a sort of concrete. The west side is the hardest, and is composed of water-washed granite, boulders, cobblestones and pebbles, very closely packed, and the interstices filled with a cementing material, which, undisturbed, is very hard. It requires blasting for its removal. The east side is not quite so hard, having some streaks of bluish clay; but it is pretty thoroughly mixed with rough stones resembling in texture the "blue stone" of which

* From Transactions, Vol. IV, page 206.

curbs are made in New York. It is very compact and furnishes a good foundation, especially in a dry climate like that at Verrugas. The sides of the ravine are very rugged.

Dimensions of the Bridge.—The bridge consists of three iron piers, four iron spans and two stone abutments; in height, pier No. 1 is 179 feet, No. 2, 252 feet, and No. 3, 146 feet; the abutments are 42 feet each, span No. 1 is 125 feet, and spans Nos. 2, 3 and 4 are each 100 feet long. The total length of bridge from centre to centre of end pins, is 575 feet; of which 425 feet is made up by the four spans, and the remaining 150 feet by the three piers. The spans are all "deck," with Fink trusses.

Each pier is composed of three transverse bents, of four columns in each bent, making twelve in each pier. Each column is supplied with a cast iron foot, resting upon a firm pedestal of granite masonry; each pedestal having an area of base and depth proportionate to the nature of the soil in which it is placed. The piers are built up in sections or stories of 25 feet each. At each joint is a casting with a tenon on each end. This casting forms the joint connecting the pieces of the column, and the intersections of the latter with the transverse and longitudinal struts, and the tie rods by which the pier is braced. All columns are what is well known as the "Phœnix."

The bridge has a grade of 3 per cent. Provision is, however, made by which all wall plates, bridge seats and roller beds are level.

Laying out the Work.—There would have been considerable risk of error in locating centres of piers and abutments by direct measurement. I therefore first located them by triangulation; setting two monuments opposite each point, at right angles to the bridge line. Then finding a piece of level ground, I measured off a line; putting in stakes corresponding to abutment faces and pier centres. Over this was stretched a small flat steel wire, with one end made fast, and the other passed over a pully, and having a weight attached to this wire, vertically over each stake, a tag was secured. The whole apparatus was then taken down and the wire stretched across on centre line of bridge. Next, by setting up the transit opposite each point, sighting to these tags, and then plunging the telescope, an accurate correction of distances was made.

Erection of the Iron-Work.—As the original plans and apparatus for erecting the iron-work have been described in other articles and are pretty well understood by mechanical engineers, it is unnecessary to speak of them here. I believe the only difficulty in using them, would have arisen in getting the material for the piers and the two middle spans down into the bottom of the ravine, and then having to hoist it all to the

various positions the pieces were to occupy. I do not think that there could be the least doubt as to their safety. They were, however, abandoned and my own plan (as below described) adopted.

A sketch (Fig. 1) shows a side elevation of the work in progress; together with the cables, temporary span, &c., and an other (Fig. 2.), a

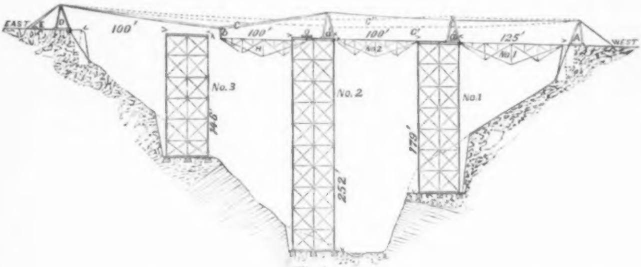


Fig. 1

transverse view of the top section of one of the piers, showing end view of temporary span. *A* and *D* are two wooden towers, standing on the ground just back of the abutments, and having a height of 30 feet. These towers supported two sets of wire cables—one set vertically over each truss line. The ends of the cables were securely anchored to sticks of timber set in the ground outside of the towers. The position of the cables at the beginning of the work is shown by dotted line *c*.

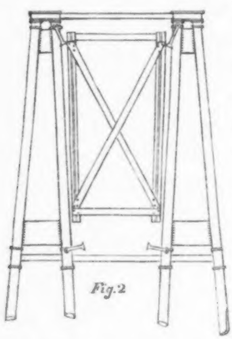


Fig. 2

Each cable was supplied with a traveler, *b*, to which was attached a set of block tackle, whose "fall" end was passed over tower *D*, and on to the drum of a winch at *E*. When the tackle was loaded, the strain on the fall was sufficient to haul the traveler out. A tail rope passed from the back end of the traveler over the top of tower *A* and around a cleet, near which a man was stationed for the purpose of paying out or hauling in as was required. *a′* and *a″* show the positions of a third tower used as the work progressed, for the purpose of shortening the span of cables; *c″* and *c″* the respective positions of the cables. *H* represents the temporary span used to support the iron-work of permanent spans while they were being put together. This temporary span was not used till after all the piers and span No. 1 were completed. It was constructed as follows:—Chords and posts of yellow pine; chains of round iron rods; lateral and diagonal

onal bracing of wood. Its width was such as to allow it to pass between the inner columns of the top section of the piers, by leaving out the diagonal tie rods of the section as shown (Fig. 2).

The materials for the bridge were unloaded from the trains at the west or Lima end of the bridge.

In erecting pier No. 1, the columns, struts, ties, &c., were moved on "push cars" to the abutment, where they were put together—one pair of columns at a time with their joint, castings, transverse strut and tie rods. A piece of plank was lashed across the lower ends of the columns to steady them. The blocks were then lowered from the travelers and made fast to the upper ends of the columns; when the men at the winches raised the load clear of the abutment, the tail ropes were paid out, the columns thus carried out over the pier and set standing in position ready for bracing.

After pier No. 1 was complete, span No. 1 was laid in position, on a scaffolding built up from the ground. Tower *a'* was then set up, and the material for the other piers run out and put together in front of *a'*. Pier No. 3 was erected next, and pier No. 2 last.

The three 100 foot spans were laid on the temporary span. As soon as one of these was swung up, the temporary span was moved forward to the next opening; the forward end being carried by the travelers, and the back on the iron-work. This operation was repeated for each of the spans.

The time taken to erect the iron-work was three and one half months. Everything worked satisfactorily. Not a man was seriously injured; and the only casualty was the falling of one piece of column by getting loose from the strap. It struck on a stone and was badly broken; but was, however, repaired in such a way as to be as strong as ever.

The excellent manner in which the work went together is evidence of the great care taken by the constructors* who furnished it, of their attention to the minutest details of the plans, and of their thorough inspection of the work.

* The Baltimore Bridge Co.

CXX.

NOTES AND SUGGESTIONS ON THE

CROTON WATER WORKS AND SUPPLY, FOR THE FUTURE.

A Paper by BENJAMIN S. CHURCH, C. E., Member of the Society.

READ FEBRUARY 2d, 1876.

NOTE.—The following comments are not intended to detract from the merits of one of the greatest achievements of modern engineering, or in anywise to underrate the masterly ability displayed and remarkable success achieved, in the plans and construction of the Croton Aqueduct. The wonder is, that so few oversights occurred to challenge criticism, and now, after the lapse of over thirty years, these suggestions are simply offered to carry forward to fuller perfection, the work as it at present stands.

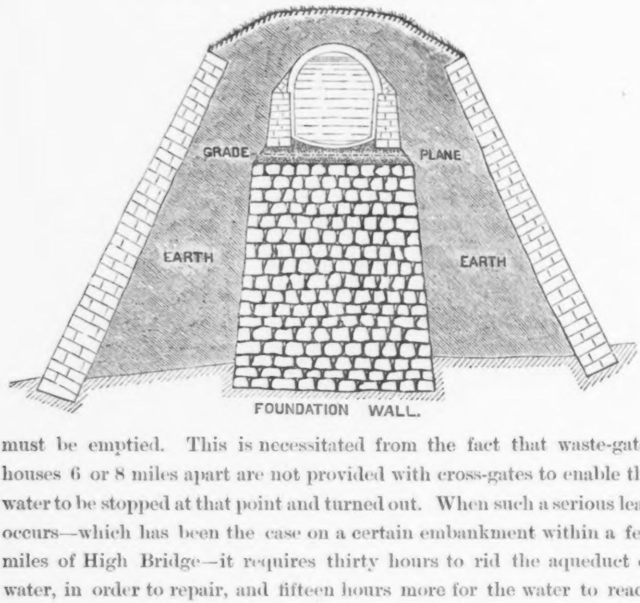
PRELIMINARY.—The Croton Aqueduct is constructed of hydraulic cement masonry and lined inside with brick. The sketch on next page shows in section the general form, which is slightly varied in earth and rock tunnels.

The maximum grade of the aqueduct is 1.1088 feet per mile with the following variations, viz.: at Croton dam, where for 5 miles the fall or grade is about 7 inches per mile; at High Bridge and Manhattan valley where the water is conducted across in pipes, and a slightly increased fall given to overcome friction, and from Manhattan valley to the reservoir in Central Park, a distance of 2 miles, where the grade is 9 inches to the mile. A considerable portion in crossing streams and ravines, about 5 miles in the aggregate, rests on rubble stone foundations, built up without mortar, and upon these one foot of hydraulic concrete was laid, forming the grade plane sustaining the aqueduct structure. Entirely covering this and its foundations, are earth embankments with outside sloping walls, the object being protection from frost, also to assist in balancing water pressure in the aqueduct.

Now, on all these embankments, one hundred and twenty-five in number, varying from 100 to 14 000 feet in length and from 10 to 40 feet in height, the masonry enclosing the column of water has not proved of sufficient strength to resist lateral hydraulic pressure of full water flow, and the friction from minute but constant movement always existing in

dry foundations. In some places, these foundations have settled and disturbed the even bed of support of the aqueduct, especially as the tendency of this settlement is to spread the wall; consequently on all these embankments between Croton dam and the city, the aqueduct has split longitudinally through top and bottom, being torn asunder by the above-mentioned forces, causing leaks of more or less serious nature. Some of these leaks during a sudden and severe change of weather become so alarming as to make it necessary to shut off the water at Croton dam and empty the aqueduct for repairs, which can only be made inside. Whenever a leak occurs, be it near the upper or lower end, the entire aqueduct

A CROTON AQUEDUCT EMBANKMENT.



must be emptied. This is necessitated from the fact that waste-gate-houses 6 or 8 miles apart are not provided with cross-gates to enable the water to be stopped at that point and turned out. When such a serious leak occurs—which has been the case on a certain embankment within a few miles of High Bridge—it requires thirty hours to rid the aqueduct of water, in order to repair, and fifteen hours more for the water to reach that point again after being turned on at Croton dam; thus making a loss of forty-five hours, exclusive of the time taken for repairs, when three hours would suffice were there cross-gates as above suggested.

These forty-five hours involve the loss of 220 000 000 gallons to the city in that space of time. The want of strength on these embankments has become more apparent because of the rapidly increasing use of water

demanding greater depth of flow in the aqueduct, which entails corresponding increase of lateral pressure, tending to reopen fissures that have been repaired. The outside protection walls have been raised and strengthened, and the embankments more thoroughly drained, which, to a certain degree, has prevented the further settlement of foundations and put the aqueduct in somewhat stronger condition than in former years, to sustain its increased burden. A thorough renewal of the broken parts would, however, require a longer stoppage of water supply than the city storage now allows; therefore, repairs heretofore made, although the best that conditions permitted, have been temporary and imperfect. Had the bottom of the aqueduct, instead of being formed of 15 inches of concrete and 4 inches of brick, been made 3 feet thick of solid masonry lined with 4 inches of brick and correspondingly increased thickness of side walls, these longitudinal ruptures would hardly have occurred, as therein would have been contained requisite strength on embankments where alone the aqueduct has proved weak.

For some years past the constancy of supply has been maintained by the following expedients, viz.: so soon as a leak appears, sawdust, fine sand or loam mixed with water into a paste, is prepared and dumped into the aqueduct at an opening above the leak and carried down by the current and drawn into the fissures, when it swells and chokes the leak temporarily until some change of temperature or water pressure loosens it, when the process is repeated. By this means, leakage being kept out of these foundations, the movement is much checked, which frequently obviates the necessity of shutting off the water for inside repairs, unquestionably resulting in distress to the city. Increasing vigilance and care have been required on the part of the engineers and their employees to keep up the daily supply.

The city consumption has so nearly reached the carrying power of the aqueduct that it can but slowly refill the reservoirs when drawn down. Stoppage occurring for inside repairs, which occupy three days and nights, draws the water down 8 or 10 feet ; and this amount of pressure taken from service pipes, is a loss sufficient to deprive upper stories of water, over a considerable area of the city. Under present conditions, should an immediate succession of repairs, such as have occurred, become necessary, the reservoirs would be so reduced that consequences might be disastrous.

In the plans of the reservoir gate-houses, an important change could be made which would relieve the present embarrassment in

making repairs, and better regulate city supply immediately following. It should be borne in mind that a full reservoir is now necessary to give full pressure to the city, its high water line being on a level with the top of the aqueduct. The present gate-houses are arranged for the aqueduct to deliver into the reservoir basins, and the main pipes conveying water to the city are supplied from these basins only. Hence every foot lost in the reservoirs is taken from pipe service pressure, only to be regained by refilling the reservoirs. The inconveniences occasioned by this, could be obviated by constructing waterways with gates arranged to enable the water to be turned into them directly from the aqueduct and fill to full height the gate house chambers supplying the main pipes. Turning on full water head after repairs would thus ensure its immediate pressure throughout the city, and the surplus beyond city consumption could flow over weirs and slowly refill the reservoirs at exactly the present ratio. By this means the city could be supplied directly from either the aqueduct or reservoirs as occasion required, and a much larger amount of storage made available. If the city used all the conduit could furnish, this arrangement would, of course, be inoperative, but that need not, and ought not to be, when means are at hand to prevent, as suggested further on.

Pipe Distribution.—Comparatively uniform distribution of water can be as readily ensured to large as small cities, if they are divided into districts, each district mapped in reference to altitude and provided with a main proportioned to its area. These mains must be so arranged that by opening connecting gates, water can be turned from one to another when occasion requires, either for relaying, repairing pipes, or to increase supply for fire.

When Croton water was first introduced, the mains laid were larger than necessary at the period, and the supply of water so abundant that a very general plan of distribution sufficed. Under present conditions, but small attention has been given to separating the circulation of localities differing in elevation, hence the cause of unequal supply. The growth of New York has been scattered and irregular, and the power to order pipes laid for newly built localities was formerly in the jurisdiction of the Board of Aldermen. Small street service pipes were connected here and there with the nearest mains, without reference to a well planned system, and it has resulted in a most complicated network of pipe connections delivering without uniform pressure and difficult of control.

Reiterating the statement that the city is consuming nearly the maximum supply of the aqueduct, the question arises how to provide for the present and immediate future? The city average steadily increases, and there is no possibility of increase by the present aqueduct. It is true the falling of the reservoir regulates, in some degree, the use of water by diminishing the pressure, as previously stated; but this diminution is attended by evils that deserve serious consideration.

The stoppage of flow in upper stories is, in a sanitary point of view, disastrous. On floors where hand basins and closet traps are apt to be deprived of water, direct communication with the sewers is established through drain pipes. These pipes act as chimneys to draw up the poisonous gases, productive of diphtheria, typhoid and other fevers; or should the same water remain for any length of time in the traps of basins and closets it becomes charged with these gases and transmits them to rooms to breed pestilence. The difficulty in obtaining full pressure in case of fire, with all the attendant danger and pecuniary loss, need not be enlarged upon.

The true alternative to counteract the evils enumerated is to stop the abuse of the water privilege. An unreasoning impression seems universal among our citizens that water should be free as air and sunlight; but they are oblivious of the fact that Croton water embodies a vast amount of treasure—costing the citizens of New York $6 000 per day, or over $2 000 000 yearly.

Cause and Effect of Waste; Means of Prevention.—To prevent waste, furnishes the only solution of this problem. Even after the construction of a new aqueduct, a matter of four or five years, which necessitates increased pipe service circulation to deliver the increased supply, the growing habit of reckless waste on each individual in a population constantly enlarging, is like a disease beyond the power of any but temporary remedies, and future years would undoubtedly exhibit a recurrence of present troubles if this habit be not checked.

It is generally supposed that the increased consumption of water has arisen from growing manufacturing interests, and the pumping of water to supply buildings in the lower business portion of the city, which now contains structures double the height of former years. But business houses and manufactories are closed on Sunday, with few exceptions, and use little or no water; yet the reservoirs gain on that day only 6 000 000 gallons, or thereabouts, which is a marked indication that it is in dwelling places and private residences, the principal waste occurs.

Standard authorities have fixed the following data, as a basis for estimating the quantity of water required for abundant supply of cities and towns—minimum 20, and maximum, 40 gallons, per head per day. A large estimate of the inhabitants of New York, including transient visitors and people living out of the city, but transacting business within its limits is 1 100 000 persons; whence the maximum supply would be 44 000 000, and the minimum, 22 000 000 gallons per day. Now the average daily consumption of New York is 114 000 000 gallons, which is more than two and one half times the maximum, and five times the minimum supply which should amply suffice. The city of London, containing three times the number of inhabitants of New York, daily uses 108 000 000 gallons, thus verifying the above estimate.

With us, there is absolutely no check upon reckless consumption, save in the single instance of steam boilers. These are rated by horse power; yet the establishment using the boiler, may waste at discretion elsewhere through the building, and often much more is used or wasted than is required for the engine, with no possibility (without meters) of measurement or discovery by the authorities.

When the temperature becomes suddenly cold, the water is set running in every house to prevent pipes freezing, turning into the sewers nine or ten millions of gallons and drawing down the reservoirs 3 inches in a night, to recover which, requires three days of mild weather. This amounts to forcing the city government to protect private plumbing, aside from bringing possible danger and disaster back upon households, in robbing upper stories and creating a short supply in case of fire. There are other sources of waste—such as drawing more water than the occasion demands—of themselves insignificant, but multiplied by thousands of similar instances, they assume formidable proportions. Tanks are often so arranged that after being filled at night, when the pressure comes, the water goes on running because no float valve is furnished to stop the flow, and this not only in dwellings, but in factories and wherever water is used in large or small quantities.

Were each consumer compelled to pay proportionately to consumption, a less expensive method than letting water run would be devised for protecting plumbing. The house main connecting with the street would be laid below the reach of frost, and pipes carried up in the interior, instead of against outside walls, where plumbers, in their own interest, prefer to place them; or they would be encased in charcoal, sawdust, plaster of Paris or any non-conductor; self closing faucets would be used,

causing the water to cease running when the hand is removed. Owners of docks, sugar houses, manufactories, and other places of large consumption, would immediately resort to similar devices to prevent carelessness which would result in personal expense.

The effect of preventing this enormous waste, which (aside from a large percentage for leakage in street pipe service) is estimated to be fully double the amount of water required for use, becomes important because of another law involved than that relating to pressure from a full reservoir. It is the law concerning "the flow of water in pipes under uniform head, viz. :—velocity and pressure becomes interchangeable terms ; as velocity increases pressure diminishes until it disappears in velocity, and vice versa." Thus velocity being checked, pressure is regained.

In the application of this law, stoppage of waste (equaling say one-half the supply) diminishes velocity one-half, thus doubling the average pressure throughout the city. The relief of the aqueduct of excessive strain which it now sustains without intermission, dangerously increasing its liability to rupture and leakage, should not be lost sight of in this connection.

Unquestionably the universal adoption of meters by the city is the simplest and easiest method of obviating the difficulties and dangers above enumerated. Meters should be considered in relation to their influence on the water supply as reducing the load carried by the aqueduct and in keeping the reservoirs filled, diminishing velocity of flow in the pipes by checking waste, thereby giving ampler pressure through the city. It is a mistake to view them only as of interest to inventors and speculators, or from the standpoint of increased revenue from water payments which have always accrued from their introduction. Although of great relative importance, the paramount urgency for immediate measures to counteract the present unsafe condition of our water supply should outweigh minor considerations of unreasonable prejudice against their use.

There are various meters existing, to meet every difference of requirement, either for large or small consumption, high or low pressure. Water can be measured by meters with more accuracy than gas, which is compressible. A cubic foot of gas represents more or less according to the pressure brought to bear, while no amount of pressure will sensibly alter the bulk of water. If gas consumers neglect to turn off a high pressure in the cellar, a third more gas may be allowed to flow through burners than is necessary to give the same or even better light. With water this variation is not possible, the meter being far more reliable and honest.

It is urged that payment for water by measurement might lead in some cases to uncleanly, penurious and unhealthy economy. To prevent this, a law providing liberal amount for dwellings according to their size, can rate taxation by the present system. But beyond this allowance for legitimate use, full charge should be made per cubic foot. Having to pay regularly for sufficient amount, whether used or not, no advantage would be gained from mean economy, while waste would involve a pecuniary responsibility. The cost of metering the city would amount to between two or three million dollars, but it would be refunded the city within a few years by increasing the water revenue.

If this method of preventing waste be not resorted to, the only alternative would be to procure an additional supply of water by the construction of a new aqueduct. That a new aqueduct, separately located, and independent of the present one, will be ultimately necessary, seems unquestionable. The present aqueduct has been strengthened to the utmost, of late years, but the increased load it is called upon to carry, as before stated, renders it liable to a succession of mishaps such as have formerly occurred, at a time when the situation was far less critical than now, in which event water famine would inevitably result. The city of New York can well afford and should take immediate steps to protect itself from such possible calamity. During the storms of last July, when the Hudson River Railroad and Albany Post Road were rendered impassable, land slides occurred on the aqueduct between Tarrytown and Sing Sing, and had the fury of the storm struck one of three places above, a portion of it might have been seriously damaged. In 1865, during a similar storm, the rush of water carrying débris, choked the culvert under a high embankment, which was cleared at great risk of life, only in time to prevent water damming to the height of 30 feet against the aqueduct, to the imminent danger of the structure.

From the foregoing statements it is evident that action is imperative to provide for emergencies which, if not yet upon us, are none the less threatening. Meters should be applied, or a new aqueduct constructed, without delay; the former appears to be the cheapest and most available resource, while the latter must ultimately be resorted to, and fortunately the Croton river offers unlimited supply for all future demands.

AMERICAN SOCIETY OF CIVIL ENGINEERS.

INCORPORATED 1852.

TRANSACTIONS.

CXXI.

LEVEES,

AS A SYSTEM FOR RECLAIMING LOWLANDS.

A Paper by George W. R. Bayley, C. E., Member of the Society.

Presented June 10th, 1875.

"Levee (*French*), for raising, removing, taking off, breaking up; embankment, embanking, bank, causeway; mole; swell;—*faire une levee*,—to embank; to make a stand;—*levee d'un siege*,—raising a siege; *levee de terre*, embankment, mound."

Levees, or embankments, or dikes as adjuncts to systems of reclamation, drainage and irrigation, have been in use since the very earliest times.

The Egyptians, Assyrians, Babylonians, Romans, Hindoos, Chinese and other nations, built dikes and dams for reclamation, and leveed canals for drainage, irrigation and navigation. It has been said that "irrigation was the first application of science to agriculture;" but, it would seem that reclamation must have preceded irrigation and drainage. It is said that the ancients "excelled in the management of dikes and dams," and therefore they must have known the value of the fertile soil peculiar to alluvial plains, and its luxuriant productiveness when reclaimed, drained and irrigated where necessary.

The remains of ancient works, built for purposes of reclamation and irrigation, and their vast extent in some countries, excite our wonder. With compulsory labor, under despotic governments, it was practicable to do then what would not be undertaken now. Herodotus says that Menes the first known king of Egypt, about 2 700 B. C., "founded the city of Memphis, after he had diverted the course of the river Nile, by raising a dike,"* probably to close an old channel, and it seems to be well established that levees, to confine the waters of the Nile and regulate irrigation, were in use at least so far back as the reign of Menes. During

* Encyclopediæ Britannica; Article—Egypt.

each successive dynasty afterwards, the system was elaborated and perfected. It is probable that for many centuries before the age of Menes —or the earliest historical epoch—the work of reclaiming and cultivating the Nile valley lands had been progressing, and that it increased with the population and as the necessities of the people required more land for cultivation.

The famous lake Moeris, and the great canal known as the Bahr-Yoosuf, or river of Joseph, and other works, including most of the raised mounds upon which cities and villages were built, were constructed when Egypt had become an empire. Sesostris, it is said, dug canals all over the delta of the Nile. Lake Moeris and the Bahr-Yoosuf, (the latter, which extended for about 350 miles parallel with the Nile, was, probably, for the most part, at least, a former channel of the Nile), were used for navigation as well as for regulating and prolonging the irrigation of the adjacent districts, by retaining water after the subsidence of the annual river floods.

"The delta of Egypt is a nearly level plain, richly cultivated, and varied alone by the lofty, dark-brown mounds of ancient cities, and the villages in groves of palm trees, standing on mounds, often, if not always, ancient. We sometimes see groves of palm trees besides those around the villages, but other trees are, except in some parts, rare."*

The annual flood season of the Nile occurs, generally, with remarkable regularity—the greatest height being reached about the time of the autumnal equinox—but it sometimes fails and at other times is excessive. Variations of a few feet in the rise of the Nile are of the utmost importance to the Egyptians, for "low inundations always cause dearths;" successive failures (such as incurred "for seven years—A. H. 457—in the reign of the Fatimee Khaleefeh El-Mustausir Bi-llah, when there was a seven years famine,"*) a total loss of crops, and excessive inundations, disease and destruction of property.

During the excessive and extraordinary flood of 1874, it is said that 200 000 laborers were employed to strengthen and maintain the levees of the Nile and its valley. The culture of cotton was introduced into Egypt by Mohammad 'Alee, and now receives much attention. Egypt exported 264 880 bales of cotton to England in 1871, and every effort is now exerted to increase this product. The culture of cotton requires the exclusion of the Nile water of overflow, from the cotton fields, by levees.

M. Regnault says: "On the banks of the Nile, the mud contains much sand, and, when it is carried by the waters of inundation to distant

* Encyclopediæ Britannica; Article—Egypt.

tracts, it loses on its way a quantity of sand proportionate to its distance from the river; so that, when the distance is very considerable, one finds the argillaceous matter almost pure."

The Nile valley lands being alluvial and annually overflowed, there is a slow but regular increase in the height of the land so inundated. The average of this increase of elevation—as determined by measuring down to the bases of ancient monuments partially buried—is stated to be from 4 to 4½ inches in a century. The high water line of the Nile keeps pace with the elevation of the delta lands, which is the usual effect of outlets in sedimentary rivers.

Various projects have been undertaken of late to better the condition of Egypt. The most promising of these was the construction of a barrage, or dam of masonry, across both branches of the Nile at the point (head) of the Delta, in order to regulate the inundation, and thus to render the country more fertile and easy of cultivation. This great work was in 1846 commenced under the government of Mohammed 'Alee, and continued under that of Ibraheem Páshá. Abbás Páshá ordered it to cease, but it is said that "Sa'eed Páshá, the present governor,* intends to complete it. There is no doubt that, if successful, this barrage would produce the most happy results."†

This barrage was found to be defective when completed. A 6 feet head of water moved a part of it, whereas it was designed for the maintenance of a 16 feet head. The present Khedive has ordered its repair. By means of it, while navigation around the dam is provided for by locks and the passage of water by numerous sluice-gates, the river above is to be maintained at a high level, and a prolonged irrigation thus secured.

For a distance of 500 miles above Cairo, the Nile valley averages 10 miles in width, and is divided into a series of basins by means of levees, along the river front as well as away from it. Near Cairo the river levees are from 12 to 15 feet in height, 12 feet in width at top, and but slightly elevated above the high-water line. The water to moisten and enrich the soil is not permitted to flow unregulated over the river banks, but is conveyed by canals and sluices to where it is wanted; it is there retained within leveed areas, so long as wanted, by closing the outlets. Egypt being a rainless country—or nearly so, except near the sea-coast—without forests, irrigation and the fertilization of the soil by the mud-bearing waters of the Nile, is an indispensable necessity. With regular irrigation, independent of the flood season, three crops annually may be

* 1855. † Encyclopediæ Britannica; Article—Egypt.

cultivated. Improved pumping machinery is now much used in Egypt for this purpose. Much more dependence is placed in methods of elevating water now than formerly, and less in the canals and reservoirs, which are neglected and suffered to go out of repair.

It is probable that the system of leveeing had its origin in Egypt. The Assyrians and Babylonians, in the valley of the Euphrates and Tigris also adopted a system of reclamation and irrigation by means of levees and canals, and of leveed areas or basins, and reservoirs to enable them to cultivate their river alluvial lowlands. The teeming population of Nineveh and Babylon, in ancient times, required all, that these fertile alluvial plains under the most elaborate system of agriculture, could be made to produce. In that dry climate, this could only be accomplished under a system of leveeing and irrigation.

The valley of the Euphrates, like that of the Nile, had been subject to annual inundation, but Queen Semiramis, it is said, "prevented the overflow by the erection of stupendous mounds or dams along the banks of the Euphrates, and henceforth the land was irrigated by hand and by engines. The entire territory was intersected, like Egypt, by numerous canals, the largest of which could be navigated by ships, and stretched from the Euphrates to the Tigris."*

It is related that Queen Nitocris caused to be excavated, above Babylon, an immense reservoir, and that she turned the Euphrates into it, temporarily, in order that piers of stone, for a bridge to connect the two portions of the city of Babylon—built on each side of the river—might be constructed in the river when so drained. Along both sides of the river, where it flowed through the city, walls of baked brick, provided with brazen gates opposite the streets, and connected with the great outer walls, served as levees, and the banks of the river were lined with burned brick.

Cyrus took Babylon by diverting the Euphrates into the great Nitocris reservoir, at night, thereby enabling his army to march along the river bed, from above and below, to and through the street river gates into the city; the river gates having been left open during a great feast and carousal made by Belshazzar, the king.

In China the immense and crowded population, of necessity is compelled to cultivate every available square foot of ground, and to make the best possible use of every means of increasing its production. Rice is the great staple crop of large portions of China, and this grain requires for its cultivation leveed areas and irrigation. The vast alluvial plains

* Wheeler's Life and Travels of Herodotus.

of the Yang-tze-Kiang, or Blue river, and of the Hoang-ho, or Yellow river, are leveed, irrigated and cultivated. M. Huc says: "The maintenance of the dikes on Yellow river is intrusted to a special board, which forms in the provinces of Tchi, Chan-toung, and Honan, a body independent of the provincial administration."

In Hindustan, owing to the suddenness and short continuance of the monsoon rains, and the long continued droughts, it has always been necessary to resort to irrigation by means of dikes, dams, canals and reservoirs, for the cultivation of the vast sterile upland tracts of country in northern, middle and southern India, which were barren only for want of irrigation.

Speaking of the "tank irrigation system" of Madras, Mr. Smith says: "The extent to which it has been carried throughout all the irrigated region of the Madras Presidency is truly extraordinary. An imperfect record of the number of tanks in 14 districts shows them to amount to no less than 43 000 in repair and 10 000 out of repair, or 53 000 in all. It would be a moderate estimate of the length for each, to fix it at half a mile, and the number of masonry works, in sluices of irrigation, waste-weirs, &c., at 6 as an average." These data, only assumed to give some definite idea of the extent of the system—would give close upon 30 000 miles of embankments, sufficient 'to put a girdle around the globe' not less than 6 feet thick—and 300 000 separate masonry works. The whole of this gigantic machinery of irrigation is of purely native origin. Valleys are taken possession of, and the natural drainage lines flowing through them are checked by embankments sufficiently long to close the gorges, and sufficiently high to retain a volume of water proportioned to the areas of irrigation situated below them. Descending steppes of land are occupied by a succession of reservoirs, the higher feeding the lower from its surplus supply, and the whole forming one connected scheme of irrigation. Dry basin-shaped hollows have banks carried round their ridges, and supplies were introduced from adjoining rivers by means of special canals; or long slopes, where the fall is considerable, have portions embanked more or less regularly on three sides."

Among the most ancient of these tank reservoirs he mentions the "Ponairy, in Trichinopoly, with its embankment of 30 miles in length, and probable area of 60 to 80 square miles, now lost to the community, and the Veeranum tank, with its 12 miles of embankments and 35 square miles in area, happily still in full operation." He considers the Chumbrumbankum tank as "one of the finest in the Madras Presidency."

Its area is 9½ square miles; its volume may be estimated at 3 000 000 000 cubic feet of water. It maintains a sheet of rice cultivation nearly 10 000 acres in extent.

Mr. Hewson says that one of these tanks, constructed in the island of Ceylon, was formed "of huge blocks of stone, strongly cemented together and covered with turf, a solid barrier 15 miles in length, 100 feet wide at base, sloping to a top width of 40 feet, across the lower end of a spacious valley."

According to Mr. Smith, the Mogul emperor, Feroze Toghlak, in the 14th century, built "50 dams across rivers to promote irrigation, 30 reservoirs for irrigation, 150 bridges, 100 public baths," &c., and the first canal of which there is any record in north-western India.

The British Government, since its conquest of Hindustan, has enlarged and improved the systems of irrigation by means of tanks and canals throughout all India. The Great Ganges canal, commenced in 1848, was to be, with its branches, 900 miles in length, and capable of furnishing irrigation for about 4 500 000 acres of land.

In India, levees or embankments, carried across the bed of a river or a valley, are termed "bunds."

In Italy, the system of leveeing or embanking rivers, and of reclaiming lands for cultivation, has been in successful operation for many centuries. The Adige, the Tiber, the Arno, Reno, and the Po—particularly the latter—are well known examples. Paul Frisi, in the preface to his work on "Rivers and Torrents," printed at Florence in 1770, claimed that "hydraulic architecture arose, advanced and almost attained perfection in Italy, where they have written on every point connected with the theory of torrents and rivers, the conducting and distribution of clear and turbid waters, the slopes, the directions, and the variations of channels, and, in a word, on the whole range of hydromety and hydraulics."

We obtain from Frisi's work, very interesting information respecting the observed effects of leveeing the Italian rivers, some of which, though undoubtedly reliable, correct and applicable to all sedimentary rivers, is not yet known or appreciated by many modern engineers. The Italian engineers demonstrated that torrents, which carry down stones and gravel, cannot be successfully circumscribed by or between levees or dikes; for their beds will rise and continue to rise, by the accumulation of stones and gravel in them ; that the lower portions of rivers carrying and flowing through sand and earth only, which make and shape their own beds and banks, can be leveed safely, without elevations of their

beds or surface as the result of the increased quantity of water confined by levees to the channel; that "derivations," or outlets, are useless for the purpose of permanently lowering the flood line in such portions of a river, and that a division of the waters of such a river into more than one channel, results in an elevation of the beds and high-water lines of the divided channels.

Frisi says: "The Po, which, formerly divided into several branches between Placentia and Parma, had converted a considerable part of Lombardy into a marsh, has been circumscribed by dikes and confined within a single channel of a suitable depth," without any increased elevation of surface; "whereas the Great Rhine, divided and subdivided as it is in Holland, has considerably elevated the bottom of its bed; and, whilst it renders the situation of the adjoining lands daily worse and worse, is threatening them incessantly with utter destruction."

"The first division of the waters of the Rhine was begun under the Roman generals Drusus and Corbulo; it was afterwards continued in later ages by a great number of subdivisions. This great multiplicity of channels, though productive of very great advantages to the navigation and commerce of Holland, draws after it very fatal consequences. The waters, divided into so many branches, lose the rapidity and strength which are required to sustain and push forward those heterogeneous substances which they transport. The constant rising of the bottom renders the draining of the waters from the fields more difficult, increases the expense of the necessary embankments, and always augments the damages which these extensive lowlands suffer, when the dikes break. To secure that part of Holland which lies between Rotterdam, Utrecht, Amsterdam and the ocean, it was proposed, in 1754, to form in the Leck, which is another branch of the Rhine, a cut with 16 sluices, by which a part of the water, should be discharged into the Mernva, which is the junction of the Meuse with the Wahal." The engineer Genneté, says Frisi, opposed the proposition to make an outlet, claiming "that the proposed alteration would avail nothing towards the diminution of the height of the floods;" and advised, "in lieu of it, to remit all the waters of the Great Rhine in the ancient branch of the Issel, and in this manner to conduct them by the shortest road to the sea." He maintained that "by the union of all the waters, their rapidity would be increased, whilst the amplitude—width—of the sections would continue the same; and that, in consequence, the waters would have more strength to deepen their bed and to prevent those deposits that are successively made in it. He supports his opinion by the directly contrary effect produced from the

actual divisions of the Rhine; and he adds, that this river, before it is divided in Holland, receives at Mentz the Mayne, whose flow is nearly as great, without its being possible to observe any perceptible difference in the dimensions—width—of its bed. The Samoggia and the Lavino, in Italy, running near each other, and having almost the same course, their floods always come down at the same time. Thus, although the quantity of water is increased nearly one-third in the Samoggia, after the injunction of the Lavino, as has been already said, and although the slope of the bottom is considerably diminished in the Samoggia, nevertheless the height is less, and the whole section very little larger than before. The operations carried on in the Gaina were not less exact; yet, although it increases by nearly a half the body of water in the Quaderna, it sensibly augments neither its height, nor the magnitude—width—of its sections. We have, besides, several other examples of running streams considerably augmented, in quantity of water, without any visible increase of their height or breadth."

"What has been observed in the conjunction of rivers, is also seen in their derivation—reduction of quantity by outlets—or division, where it often happens that in diverting from the principal channel a considerable body of water, that which is left behind is not visibly diminished, either in height or in breadth. The canal made by order of the Emperor Nerva, to draw off the superfluous waters of the Tiber, at the time of its greatest freshets, did not contribute in the smallest degree to prevent the inundations, as Pliny has assured us in his letters. The two relieving sluices that Vincent Viviani caused to be made in the Celone, which is a tributary of the Chiana, have caused the filling up, and the loss of the principal trunk." Speaking of the Adige, it is said "that all derivations —outlets—made in that river have only produced a heightening of its bed, and thereby rendered its floods more dangerous."

Other quotations, of the same purport, might be added, did space permit. Frisi says that "It is an hydrostatical paradox, commonly taught by Italian authors, and uniformly confirmed by experience, that you do not diminish the height of the waters in great floods by lessening the quantity of the water." His meaning obviously is, that outlets—or "derivations," as he terms them—will not permanently reduce the flood line of a sediment bearing or turbid river. Guglielmini announced that "the greater the quantity of water that a river carries, the less will be its fall," or surface slope; and he adds, that "the greater the force of the stream, the less will be the slope of its bed."

It has been taught, and generally believed, that the leveeing of the river Po has caused so great an elevation of its bed as to elevate its flood line higher than "the roofs of the houses in Ferrara," and the rapid prolongation of its mouth into the Adriatic sea. This belief is due to erroneous statements made by M. Cuvier, based upon errors of fact alleged to have been communicated to him by M. de Prony, in 1830, which statements are now known to be incorrect.

The Chevalier Lombardini, a distinguished Italian engineer, in a paper upon the "changes in the hydraulic condition of the Po," published in Milan in 1852, demonstrated that levees had not caused any elevation of the bed of the Po, and that the distance to the sea from Stellata, 16 miles above Ferrara, by the then course of the river, was 6 miles less than in 1152; consequently that no increased elevation could have been caused by a prolongation of the river's mouth. Lombardini, by careful levellings, transferred the high water-mark of 1839, the greatest flood known, from Ponte Lagoscuro, 3 miles below Ferrara, to Stellata, and thence to Ferrara, by the measured slope and along the old course of the river, and found it to be "3 feet below the surface of the ancient embankment of the Po, and 5 feet, only, above the ancient natural bank."* The flood line of the Po, in 1839, at Ferrara, was but 10 feet above the pavement opposite the palace in Ferrara, which is 1 000 feet distant from and on lower ground than the natural bank, as shown by Lombardini; who expressed the opinion that M. de Prony had only stated to M. Cuvier that the flood line of the Po was higher than "*the first floor*" instead of "*the roofs*" of the houses in Ferrara, and that "the exaggeration is due rather to Cuvier" than to M. de Prony.

In Holland, a country formed by alluvial deposits, and of sands washed up by the sea, and originally an immense sea-marsh, reclamation by means of levees, has been carried to a greater extent than in any other part of the world. Gradually, in the course of centuries, the waters of all its rivers and their labyrinth of channels, the Schelde, the Maes, the Rhine, the Yssel, have been circumscribed by levees or dikes, and immense areas of submerged land have been redeemed from the ocean.

Large portions of Holland are below the level of the sea and are protected by "dikes of vast extent, built in the course of ages, partly of huge blocks of granite brought from Norway and partly of bundles of young trees, willows, reared expressly for the purpose. These dikes stretch for hundreds of miles along the coast, and with those which line

* Report on Mississippi River, by Humphreys and Abbot.

the rivers and canals, and with the requisite sluices, draw-bridges and hydraulic works of every kind, are estimated to have cost not less than £300 000 000 sterling. They form, in so small a country, a most astonishing monument of human industry."*

The whole of Holland is a vast and intricate net-work of rivers, channels and canals bordered by levees. Destructive inundations, caused by breaks or crevices in the levees, occurring during floods in the rivers, or storms at sea, sometimes happen in Holland, but they are by no means common. Every possible precaution is used to prevent the occurrence of crevasses, and to provide the means for closing such breaks with the least loss of time.

In March, 1855, "the rivers, augmented by the snows of winter, burst through the dikes in several provinces. A fourth part of Gelderland was submerged. The embankment of the Rhine having burst in five places in Gelderland, admitted the flood where it had not extended for 150 years. In Utrecht and North Brabant, the people of many communes had to abandon their property to the waters and sought refuge for themselves on the roofs of houses and on trees."*

Extensive lakes have been formed by these inundations. "The Biesbasch, in the neighborhood of Dort, was formed in 1491, burying 72 villages under water and drowning 100 000 persons."* The lake of Haarlem was, it is said, formed, originally, by the overflowing of a river, and its drainage by means of pumping machinery was considered one of the greatest achievements of the age.†

The inundations from the sea, caused by storms, have permanently submerged extensive sections of country. The Dollort and the Zuyder Zee are remarkable examples. "The Dollart, between Gröningen and East Friesland, originated in 1277, and was greatly extended in the three following years. One town, 35 villages, and several hamlets were overwhelmed. The Zuyder Zee was formerly only a lake, known by the

* Encyclopediæ Britannica; Article—Holland.

† The Haarlem Lake dike or levee is 37 miles long, and it encloses 44 659 acres of land; 41 648 acres of which are cultivable, exclusive of the area covered by the canals and roads within the *polder*, which aggregate 919 miles in length. The lake was pumped dry by means of three immensely powerful steam pumping engines, between the months of May, 1848, and July, 1852; the working time of the pumps being but 19½ months. Including infiltration and rainfall, the quantity of water pumped out, in all, was 900 000 000 tons. The general depth of the lake, before the work was commenced, was 13 feet below the Amsterdam bench mark, which is very nearly mean tide level, or 28 inches above ordinary low water, and 31 inches below ordinary high water. The highest water within the *polder*, since the work was completed, is not allowed to exceed 15½ feet below the Amsterdam b. m.; which is 18 inches below the lowest land. It is now contemplated to drain a portion, equal to 480 000 acres, of the Zuyder Zee itself; and this will probably be done.

name of Flevo, communicating by two channels with the North Sea. Now the expanse of water is 80 miles long and from 20 to 40 miles broad."*

The highways, constructed along the summits of the dikes, and paved with hard burnt bricks, called "klinkers," and covered with sea-sand, are among the best in Europe for light carriages, and are kept in excellent condition. De Luck informs us that the "sea-banks," or levees, "on the coast of the North Sea, at the mouths of the Eyder and Elbe, extend to not less than 350 miles." The shores of the Baltic, and of the Bay of Biscay, are embanked for hundreds of miles.

The site of the city of London was once a lake bordered by marshes, through which flowed the river Thames. Some of the marshes near London are yet 12 feet below the level of high tide in the Thames. In 1478, the work of reclaiming lands by levees in England was commenced by Bishop Morton. Subsequently, Charles the First, with the Earl of Bedford and his friends, completed the work begun by Morton, and reclaimed 1 033 360 acres. About the middle of the 17th century, under Cromwell, nearly half a million of acres of morasses, fens and overflowed lands were reclaimed on the coast of England. It is said that the embankments on the coast of Essex alone exceed 220 miles. The Thames, the Mersey, and other rivers, subject to heavy freshets, are leveed or embanked. In Ireland, also, the reclamation of overflowed land has been extensively carried on of late years. In France, levees have been extensively used to protect the lands bordering upon the principal rivers. In 1858, M. Dupuit published in a small volume, a very able and convincing argument in favor of a levee system, and of its reliability as a means of preventing inundations.

Quite recently—since 1871—the levee system has been applied, on a large scale, in California, to the reclamation of the *tule* lands in the valley of the Sacramento and San Joaquin rivers. We learn, from Mr. Nordhoff, that 800 miles of levees were to have been completed, for the reclamation of tule land, in 1873, and he gives a list of tule islands containing 217 400 acres, much of which has been reclaimed and cultivated. He says that the "Yuba, the Feather, and the American rivers, tributaries of the Sacramento, have been leveed at different points for quite another reason. These rivers, once clear and rapidly flowing within deep banks, are now turbid, in many places shallow, and their bottoms have been raised from 20 to 30 feet by the accumulation of the washings from the gold mines in the foot hills. It is almost incredible the change

* Encyclopediæ Britannica; Article—Holland.

the miners have thus produced in the short space of a quarter of a century. The bed of the Yuba has been raised 30 feet in that short time; and seeing what but a handful of men has effected in so short a period, the work of water in the denudation of mountains, and the scouring out or filling up of valleys during geological periods becomes easily comprehensible." The material to build the Sacramento tule land levees with, is a sort of "tough turf, full of roots, which is very cheaply cut out with an instrument called a 'tule knife,' and thrown out upon the levee, where it seems to bind well, though one would not think it would." This turf is taken from a ditch on the inside of the levee, leaving a space of low marsh outside. The levees are usually made from 6 to 8 feet wide at top, with a slope upon the inside, but Mr. Nordhoff says:—"that experience has shown that the outside should be perpendicular." Self-acting sluice gates are used for drainage during low water in the river, the tidal range being about 6 feet. These reclaimed tule lands have proved to be extraordinarily productive.

With the construction of the Sny Island Levee, Illinois,* the leveeing of the upper Mississippi (above the Missouri) was commenced. The Sny levee is 52 miles long, contains 2 500 000 cubic yards of earth in embankment, and cost about $650 000. It reclaims 10 000 acres of land, and the value of these lands, with the improvements, it is estimated, will be $5 000 000 within three years. The success which has attended this improvement will undoubtedly determine those interested in the reclamation of other lands, elsewhere in Illinois and in the adjoining States above, to adopt the levee system. It is estimated that there are 1 000 000 acres of overflowed land in Illinois alone which can be reclaimed by levees and made into productive farms. The Sny Island levee may be regarded as the pioneer work of the upper Mississippi.

We are now prepared to consider the leveeing of the Mississippi river. The city of New Orleans was laid out in 1717, by engineer Dumont de la Tour, on the left bank and in the concave bend of the Mississippi river which approaches nearest to the Lake Ponchartrain—about 5 miles distant. The flood line of the river, at that time, was determined to be about 3 feet above the natural river bank in the bend, and De la Tour ordered a front levee to be constructed, for the protection of the future city, 4 feet high, 8 feet wide at top, and 5 400 feet long. In the rear, on the present line of Rampart street, a levee 6 feet high was ordered, and, at the sides, above and below, levees gradually increasing

* Just completed under Mr. Corthell as engineer.

from 4 to 6 feet. These levees were constituted lines of fortification, as well, and were provided with stockades and ditches, bastions at the rear angles and forts in front. They were not completed until 1726, and they were the first constructed in the valley of the Mississippi.

The Place d'Armes, now known as Jackson Square, and the cathedral and public buildings, were located opposite the middle of the city front. No perceptible change has occurred in the position of the river bank, opposite the lower side of the public square, either by accretion or caving, since 1717. Above, an extensive batture has gradually formed; below, there has been very little if any change for some distance. On the opposite or right bank of the river, a little further down, where the Belleville Foundry now stands, the river bank has also remained stationary since 1717.

Before the era of levees, the "points," next to the river, were kept, by deposits, nearly up to the highest flood line, while the "bends," or the natural banks around the bends, were, generally, from 2 to 3 feet or more below the flood line. Consequently, as the water flowed out over the banks in the bends, outlet channels existed in every bend; the well defined remains of these are plainly discernable, in very many localities, now.

Soon after the settlement of New Orleans, plantations were established along the river banks above and below the city. Every proprietor had to construct and maintain his own levee, but, as the river deposits had kept the natural banks nearly up to the flood line, small levees answered the purpose. With outlets in every bend, and a consequent reduction of current velocity in the channel to a minimum, the tendency of the river to undermine its banks at the bends, and cave in, was much less than now; therefore, the first levees were not only smaller, because the river banks were higher, but more permanent and more cheaply maintained than now.

In 1723, there were small settlements in Pointe Coupee, at Baton Rouge, near Bayou Manchac, below Bayou Lafourche, at Cannes Brulées and at Schapitoulas. In 1728, the settlements extended "for 30 miles above New Orleans," almost continuously. In 1735, according to Du Pratz, "the levees extended from English Bend, 12 miles below, to 30 miles above, and on both sides of the river." In 1743, says Gayarré, "an ordinance was promulgated requiring the inhabitants to complete their levees by January 1st, 1744, under penalty of forfeiture of their lands to the crown." In 1752, according to Monette, the settlements

were nearly continuous for "20 miles below and 30 miles above New Orleans," and, he says, "nearly the whole coast was in a high state of cultivation and securely protected from floods."

In 1770, according to Pittman, the levees still extended only "30 miles above and 20 miles below New Orleans." In 1763, France ceded Louisiana to Spain, and the policy of the latter government was not such as to foster the growth of the colony. But little progress was made in levee construction until after the cession of Louisiana to the United States, in 1803, by France; it having been ceded back to France, by Spain, in 1800.

In 1805 the settlements on the Mississippi river began "about 20 leagues from the sea," and extended almost continuously to Baton Rouge.* Above Baton Rouge, "on the west side of the Mississippi, is Pointe Coupee, a populous and rich settlement, extending 8 leagues along the river." Thence above, there were only small settlements, opposite Natchez, near the mouth of the Arkansas river, and at New Madrid. Plantations extended down both banks of the Bayou Lafourche "for near 15 leagues," to near the present town of Thibodeaux.

In 1812, Louisiana was admitted into the Federal Union, and, according to Stoddard, the levees extended "from the lowest settlements to Pointe Coupee on one side, and to the neighborhood of Baton Rouge on the other, except where the country remains unoccupied." In 1828† "the levees were continuous from New Orleans nearly to Red river landing, excepting above Baton Rouge on the left bank, where the bluffs rendered them unnecessary. Above Red river they were in a very disconnected and unfinished state on the right bank as far as Napoleon. Elsewhere in the alluvial region, their extent was so limited as to make it unnecessary to mention them. In 1844, the levees had been made nearly continuous from New Orleans to Napoleon, Arkansas, on the right bank, and many isolated levees existed along the lower part of the Yazoo front. Above Napoleon, few or none had yet been attempted." As stated by Prof. Forshey: "Between the years 1850 and 1860 the levees of the right bank, from Cape Girardeau down to near the mouth of the Arkansas river, were built piecemeal, but finally were nearly continuous, leaving intervals of less than 40 miles, in the aggregate, when the war of 1861 ended all improvements."‡

The process of levee construction began at New Orleans in about the

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* Humphreys and Abbot—Extracts from State Documents.

† Humphreys and Abbot. ‡ Transactions, Vol. III, page 270.

year 1720, and it progressed, gradually, downwards for about 70 miles, and upwards nearly 1,000 miles, during 150 years. Obviously, as outlets existed in every river bend—for the natural bank was overflowed from 2 to 3 feet or more in every bend before it was leveed—and these were successfully closed during the period extending from 1720 to 1860, if the effect of closing outlets and confining all the water to the river is to raise the flood line, there should be abundant evidence of such increased elevation in the lower river; notwithstanding that crevasses have occurred—because of neglect of or defective levees—during every flood year, to a greater or less extent. Every original outlet, except the Bayou Lafourche—the high water capacity of which is less than 12 000 cubic feet per second, or less than the one-hundreth part of the main river—has been closed below Red river; the Bayou Plaquemine, which discharged about 35 000 cubic feet per second, was the last closed, in 1865. No one, who is at all familiar with the subject, will contend that the crevasses of late years have been equal, in outlet capacity, to those existing, for miles in length around every bend, prior to the leveeing of the Mississippi. In fact, the outlet capacity in 1720 was beyond all comparison greater than it has been during any crevasse year since.

There is evidence that the normal flood-line of the Mississippi river, from Red river to the Head of the Passes (except where affected by cut-offs), is *not* the fraction of an inch higher now than in 1717, before the commencement of the levee system.*

The front lands about the Belleville Foundry, in Algiers, opposite the levee portion of New Orleans, are now as they were left by deposits from the flood waters of the river before it was leveed. Observation shows that, in a current, as under a wharf next the river bank, the deposits do not generally reach within one foot of the flood-line. Therefore it is altogether probable that the Algiers point was overflowed at least one foot before any levees were built below Red river. The United States Engineers, Humphreys and Abbot, determined, by accurate levelling, that the front lands about the Belleville Foundry were only 0.3 feet

* The fact that the lower Mississippi river, below its last affluent, Red river, was first leveed, and therefore first enlarged in section by and accommodated to the increased quantity of water retained in the channel by levees, explains why the leveeing of the river above has not caused any elevation of the flood-line below. Had the levee system been commenced at the head of the Mississippi alluvial formation, instead of at its foot, the result would have been different. In such case the unleveed river below, with its channel reduced by outlets in every bend, would have been unsufficient to discharge the increased quantity; the outlets would have prevented the enlargement of the channelway, and therefore the inundations would have been more extensive. To prevent injury to lower Louisiana, still, the levee system of the lower river should first be perfected and every outlet closed.

below the high-water marks of the great flood year, 1858. There are other places, below New Orleans, the elevations of which confirm the belief that the flood-line of the river was as high before levees were built as now.

Recent levellings, connecting the river flood-lines on the New Orleans side with the streets in front of the Cathedral, show the same results. Taking the water line of 1862 as a plane of reference, it was found that the bottoms of the gutters on Old Levee street, opposite Jackson Square, were only 3.4 feet below the 1862 mark; the crown of the street is but 2.1 feet below. The crown or middle of Chartres street, just in front of the old Cathedral, and one square back of Levee street, is 4.2 feet, and the gutters but 4.95 feet below. So far as can be judged, the grades of these streets do not appreciably differ from what they were in 1720, and as history informs us that the river flood-line was 3 feet at least above the natural surface next the river there in 1717, and as it is no higher there now, we may infer that the flood-line is certainly no higher now than it was 156 years ago in front of New Orleans. As before stated, the river bank opposite the lower side of Jackson Square—the old Place D'Armes—has remained unchanged since De la Tour first laid out the city.

There were several small crevasses in Pointe Coupee parish in 1862, as well as one small one near Baton Rouge, but, as the quanity of water discharged was not great, there was no general overflow. The water-mark of 1862 was 0.7 feet higher at Algiers, opposite New Orleans, than in 1858, but this was but 1 foot higher than the old natural bank there, and no higher then, if so high, as the great floods of old times must have been. In 1871, the flood-line at Algiers was the same as in 1862, but this height there, was due to a storm-tide; for the river, in that year, was not up to the 1862 mark at Donaldsonville, Baton Rouge, and other points above New Orleans. At Donaldsonville, the high-water of 1871 was 1.45 feet *below* the flood-line of 1862. In 1874 again, an extraordinary storm-tide raised the river at New Orleans about 8 inches above the 1862 mark, but this swell, caused by a wind blowing up river, did not extend more than about 20 miles above. At a point 45 miles above New Orleans, the flood-line of 1862 exceeded that of 1864 by 6 inches. At Donaldsonville, the head of Bayou Lafourche, and at Plaquemine, 30 miles above, and 110 miles from New Orleans, the water of 1862 was about the same as in 1874; the water of 1874 being 1 inch below that of 1862, at Donaldsonville.

In a recent report* to the President of the United States, it is remarked that the effect of closing the Bayou Plaquemine has been to add "about 6 inches to the normal flood-height at New Orleans." This seems to be contradicted by the facts, that the 1862 flood has not been equaled since, below the Plaquemine, and that that bayou was only closed in 1865. The continual closure of outlets for and during the last 150 years has had no such effect, although it has often been confidently predicted that the continual leveeing of outlets above, would, if not discontinued, cause the raising of the river flood-line in lower Louisiana many feet and the submergence of the lower country.

If the closure of one small outlet, discharging about 35 000 cubic feet per second raised the river flood-line 6 inches at New Orleans as alleged, then how explain the anomaly that the closure of hundreds of other outlets previously had no such effect? The Plaquemine outlet, as above stated, was closed in 1865, but the previous flood-height of 1862 has never since been exceeded at Donaldsonville, 30 miles below the Plaquemine. As before explained, the apparently greater flood-heights at New Orleans in 1871 and 1874, were due to storm tides on a high river, and not in either year to a rise from above.

The flood-water line of 1862, allowing it to be 8 inches below the anomalous tidal flood-water mark of 1874—which is the difference, according to the United States Signal Service Reports, and the levels and guage readings of the New Orleans City Surveyor—is just 14 feet above "ocean level," or mean tide in the Gulf of Mexico. The lowest river level in front of New Orleans, recorded on the city guage, is 0.8 feet below mean tide and 0.2 feet below mean low tide in the Gulf, at the mouth of the river, or 14.8 feet below the 1865 mark. It follows then, that inasmuch as the extreme range of 15 feet observed at New Orleans in 1735, at the very beginning of the levee system, slightly exceeded the extreme range to the 1862 mark; the high water of 1735 fully equalled if it did not exceed that of 1862, and therefore, again, that the river flood height at New Orleans has *not* been elevated since 1735, and hence not at all by leveeing up outlets.

Experience has demonstrated, here as elsewhere, that the effect of a levee system has not been to cause any increased rise of the flood line or of the bed of the lower Mississippi river, and the building of levees and their maintenance is nothing else but the permanent closure of

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* By a Commission of Engineers, on "a Permanent Plan for the Reclamation of the alluvial Basin of the Mississippi River subject to Inundation," January, 1875.

outlets and the retention within the river banks of water which formerly escaped from them. The opinion has prevailed, not only that the Mississippi flood line has been and is being elevated, but that the river's bed has been filled up and is rising, and that there is a more frequent recurrence of and more extensive overflows now than formerly. The truth is that the river is deepening and its sectional area increasing. The inundations are not more extensive now than formerly, nor, it is thought, more frequent, but they are admitted to be more disastrous, because more land has been reclaimed and cultivated, the number of sufferers is larger and the interests involved greater. The tendency of a levee system is to reduce, instead of to elevate the river flood line ; there are other causes, however, which tend to counteract this effect, among the most important of which are cut-offs and the clearing away of forests in the north and northwest.

In 1718, the next year after the selection of the site for the city of New Orleans, Xavier Martin records that there was an "extraordinary rise of the Mississippi," which greatly discouraged the new settlers. It is said: "Bienville had selected a site for a city, but the colony not having means to build dikes or levees, the idea (for the time) was abandoned." The New Orleans levee, we are informed, was nevertheless completed in 1726, but the assumed grade line was probably too low or it was built in an imperfect manner, for a great flood occurred in 1735 which inundated the city. Elsewhere along the settled portions of the river front, the levees were also broken or submerged in many places. This flood of 1735 at the beginning of the levee system, was continuous for an unusual length of time—from late in December until late in June—and the succeeding low water was remarkably low, giving a range from high to low water at New Orleans of 15 feet, or about the same as to the highest time flood-line of recent years, 1862, which is 14.8 feet.

The records of the flood years from 1735 to 1770 are wanting, but, in the latter year, a great flood occurred with its usual inundations. In 1782, there was a flood which it was said exceeded any "remembered by the oldest inhabitant." Great floods occurred also in 1785, 1791 and 1799, and during each of these years, New Orleans was inundated. The years 1809, 1811, 1813, 1815, 1816, 1823, 1824, 1828, 1844, 1849, 1850, 1851, 1858 and 1859 were marked as flood years. In 1865, 1867 and 1874 extensive inundations occurred. In 1862, the flood-line was everywhere below Cairo remarkably high, although several crevasses occurred

below Red river, their capacity was insufficient to cause any general inundation of the valley west of the Mississippi and south of Red river.

Before the era of levee construction in the Mississippi valley, the unbroken primeval forests covered millions of acres of land which are now denuded. Although forests promote and increase condensation and check evaporation, they absorb a very large proportion of the rainfall. The rapid denudation of the valley lands of the tributaries of the Mississippi must have a very important effect upon the river floods. Evaporation, it is true, is more rapid from cleared land and the condensation somewhat less than before when timbered, but it is probable that we are nevertheless, now more liable to sudden freshets than formerly. A conjunction of freshets from several of the most important tributaries makes a great flood in the main river, although the mean annual discharge may not be increased. Whether the mean annual discharge of the Mississippi estimated* at 19 500 000 000 000 cubic feet of water, is increasing or not, we have no means of determining accurately as yet. The same authority gives the average discharge of great flood years at 27 000 000 000 000 cubic feet, and the yearly amount of rainfall in the Mississippi valley, including the valleys of all its tributaries, at nearly 90 000 000 000 000 cubic feet.

The ratio of drainage to rainfall in the "entire Mississippi valley, exclusive of Red river," is estimated* at 25 per cent. The drainage ratio for the short tributaries, such as the Yazoo and St. Francis rivers, is put as high as 90 per cent., the Ohio and upper Mississippi rivers at 24 per cent., Red river at 20 per cent., and the long tributaries, which flow through a prairie country, the Arkansas and Missouri rivers, at 15 per cent. only.

It is evident that the time occupied in the flow of water is a very important element, so far as relates to evaporation and the consequent ratio between rainfall and drainage; the shorter the tributary, the greater the ratio, and the longer it is, the less the quantity that reaches the main river.

It would seem that the difference between downfall and drainage in the short tributaries at least, must increase as forests are cleared away; therefore, that in some years, the quantity of flood-water now conveyed to the lower Mississippi, within a given time, may be greater than formerly. If so, the river accommodates itself to the increase, for the flood-line is not elevated.

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* By Humphreys and Abbot.

Artificial reservoirs are inapplicable to the Mississippi river, and the natural swamp reservoirs are worse than useless for the purpose of reducing the height of the river floods. The swamp basins between Cape Girardeau and the mouth of Red river only retain water from the first rise to add to those which follow, by discharging from their outlet channels below upon and thereby adding to the flood in the river at its high stage. The river floods are thus prolonged, and undoubtedly increased by the water admitted into the swamp basins through openings or gaps in the levees above.* Generally there is a succession of freshets from the great tributaries, and if the river was securely leveed continuously, each rise or swell would pass off by itself; no one, occurring later, would overtake the one preceding.

In this, as well as in other respects, a perfected levee system would tend to lessen the danger of inundations; the river channel would be accommodated to its necessities, and the danger or liability reduced to its minimum.

Reclus, in his work, "The Earth," says that the Mississippi seems to "contradict the law of the displacement of running water," which in consequence of the motion of the earth on its axis causes all streams flowing north or south to hug the west side of their valleys. He gives a number of cases in proof of the law, but instances the Mississippi as an unaccountable or unexplained exception. It is suggested, in explanation, that the Mississippi is not, as to its tendency to flow to the west, any exception, except in appearance, to the law stated by Reclus. The river does wear away its western shore-line more rapidly than the eastern, but it cannot do otherwise than gradually excavate circular bends, of from 20 to 25 miles in circumference generally, and then cut them off, leaving them to the westward. There has been, always, an excess of overflow and of sedimentary deposits west, and by far the largest number, as well

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* A notable example of such an effect, occurred in 1874. The largest of these swamp reservoir basins extends from the Arkansas river to the mouth of Red river—the latter being its outlet channel. In 1874 many large crevasses were open in upper Louisiana and lower Arkansas, and this immense basin, traversed by the Bayou Macon, Tensas river and other drainage outlets, was gradually filled to its utmost capacity. In addition, very heavy rains caused a great flood in the Oauchita valley. About April 1st, 1874, this reservoir basin commenced discharging itself into the Mississippi through the mouth of Red river, and over the Mississippi river banks for more than 20 miles above Red river. The river rose steadily, thenceforward, 50 inches above the point it had reached previously, or to a height about 13 inches above the mark in 1862,—which was the highest before recorded at Red River Landing. During the same period, the river rose at Vicksburg but 9 inches, when it was 6 feet below its previous flood height, showing that the extraordinary rise which swelled the river, from Red river to Baton Rouge, above the highest preceding flood height known and recorded by guage, was due to a discharge out of the swamp reservoir basin extending from the Arkansas to Red river.

as the greatest bends when cut off, have been left to the west. The western portion of the valley is everywhere well filled with alluvion, and the swamps west have firm bottoms throughout the valley. Below Baton Rouge, where the river tends to the southeast, the swamps on the east are boggy and not well filled with deposits, and the large spaces covered by Lakes Maurepas and Ponchartrain are left unfilled. If the Mississippi had been a river of clear water (instead of being sedimentary), traversing a valley not alluvial, it would probably occupy the western side of its valley like other streams flowing towards the equator; but, as it is, it levees or embanks itself to the eastward by an excess of deposits west. It hugs the bluffs on the east side, down to the last one at Baton Rouge, for the reason that it could not be forced any further eastward; but immediately below the last bluff, the excess of deposits west crowded the river channel eastwards still further; the general direction thence to the present mouth being southeast. The mouth of the river having now reached very deep water in the Gulf of Mexico, and having advanced a little beyond the filling up of the gulf west, and beyond the southern limit of the western highlands, the tendency is to flow westwards by the Southwest Pass, which is now the largest channel, conveying about one-third of the whole river to the sea.

"Cut-offs" precipitate a whole river upon a lower level below the bend cut off; therefore, they lower the flood line above and elevate it to some extent, and for a time create a gorge or swell below. The velocity of the current, both above and below the cut-off, in consequence of the increased slope due to the shortening of the river, becomes much greater than before; while the caving of the river banks, because of the change in the direction and force of the current, is much accelerated. Further off, below, the increase of current is diminished and made to conform more nearly to what the quantity requires. The tendency is to increase the length of the river again by excavating and lengthening the bends, and thus to reduce the surface slope to what it was before the cut-off was made. Time is required to accomplish this, and from time to time, during this process, new levees must be built further back, on lower ground around the bends, and therefore higher, larger and more expensive levees than before. The alluvial lands bordering upon the Mississippi river slope rapidly downward away from the river bank. A fall of 15 feet within one mile back from the river is not uncommon above New Orleans. Every new levee must be made longer and stronger, because higher, opposite a caving bend; for the reason that, the ground being

lower at each new location, the additions must be made to the base of the embankments.

When the Mississippi river banks were first leveed, below Red river, embankments of from 4 to 5 feet in height only, with a crown of 4 feet and slopes of 2 to 1 were found sufficient around the bends where now levees 15 feet and even 20 feet high in some places, with 10 feet crown and slopes of 3 to 1 are needed. The new 15 feet levee contain nearly twelve times the quantity of earth, for a given length, required for the old levee.

Every "cut-off," therefore, adds very much to the cost of levee maintenance and increases the danger of inundations. But two bends have ever been cut off below, and one opposite the mouth of the Red river, although many have occurred above. The lowest—just above the Port Hudson bluff—known as the Tausse Rivierre cut-off, was done in 1722, at the beginning of the levee system. The next above, or the Raccomci, was by the State of Louisiana in 1848-9. The Red river cut-off was made by Capt. Shreve, an employee of the U. S. Government, in 1831. The distance around these bends average more than 20 miles, and the high-water-fall across their necks, more than 4 feet each; therefore, these three cut-offs shortened the river more than 60 miles, and added about 12 feet to the surface slope. Although there has been greatly increased caving and lengthening of river bends ever since, below Red river, it is not believed to be sufficient, yet, to restore the length of the cut-off, nor reduce the slope to what quantity of water flowing required.

The mouth of the Ohio river was, in 1860 (before the cut-offs, one in Arkanses and two in Louisiana, which have been made since), about 1 080 miles from the Gulf of Mexico, and the high water elevation there is 322 feet above tide water. This gives an average fall per mile to the sea, at high water, of 3.58 inches; but, the fall per mile diminishes gradually from the Ohio to the head of the Passes, and the range of rise and fall also becomes less and less, from about the mouth of the last affluent to the gulf.

From Cairo to Columbus, 21 miles, the fall per mile, at high water, is nearly 7 inches;* thence to Memphis, 204 miles, 5.25 inches; to Gaine's Landing, 225 miles, 4 inches; to Natchez, 269 miles, 3.5 inches; to Red river, 62 miles, 3 inches; to Baton Rouge, 71 miles, 2.75 inches; to Donaldsonville, 52 miles, 2 inches; to New Orleans, 82 miles, 1.75 inches; to Forts Jackson and St. Philip, 74 miles, 1.5 inches, and to the head of

* Humphreys and Abbot's Report on the Mississippi River.

the Passes, 20 miles, 1.33 inches. Though the Southwest Pass, where the quantity of water is diminished to one-third of the whole rivers' discharge, the fall per mile is increased to 2 inches, and through the South Pass, with a flow much less still, the slope is 2.5 inches.

The high water elevation above the Gulf, at the head of the Passes, is 2.8 feet, and at New Orleans, about 15 feet. The ranges, from high to low water,* are as follows:—at Donaldsonville, 24.3 feet; Baton Rouge, 31.1 feet; Red River Landing, 44.3 feet; Natchez, 51 feet; Vicksburg, 49 feet; Lake Providence, 44.7 feet; Helena, 46.4 feet; Memphis, 40 feet; Cairo, 51 feet. These, with few exceptions, are according to observations made before 1861. The extreme ranges since observed, differ somewhat in some of the localities, from those given above. Cut-offs change the range, in some cases; as at Vicksburg, where it has been reduced.

The average width of the Mississippi river diminishes very much from the mouth of the Ohio to the Passes, while the depth of channel-way increases. The average width of the river from the Ohio to Red river is about 4 275 feet at high water and 3 230 feet at low water; while the average width below Red river is about 2 700 feet at high water and 2 500 feet at low water.

Opposite New Orleans and below, the high water depth of the river, in places, exceeds 180 feet, and it generally exceeds 100 feet in mid-channel, at low water. Above Red river, the river decreases in depth as you ascend it, between Memphis and the Ohio; at low water, steamboats sometimes find too little water for navigation.†

A recent comparison of cross-sections of the river opposite Jackson and St. Anne streets, New Orleans, by Prof. Forshey, taken in 1850 and 1872, shows an enlargement of cross-section in the one case of 54 000, and in the other of 56 000 square feet. The depth also had been increased from 150 to 165 feet, in mid-channel, on one section, and about 13 feet on the other. Another section opposite the lower city district, at Louisa street, also showed a large increase of section.

There are evidences, opposite Baton Rouge, and in some other places, that the river is slightly widening itself also, as well as deepening; the levees have, of necessity, been reconstructed further back on both sides,

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* Humphreys and Abbot.

† The maximum high water discharge of the Mississippi river, during the flood of 1858 (according to Humphreys and Abbot), was per second about 1 400 000 cubic feet, at Vicksburg it was 1 245 000 feet, and at Carrollton—just above New Orleans—1 188 000 feet; with levees perfected, they estimated that the discharge would have been at Columbus 1 478 000, and at Vicksburg and points below, 1 430 000 cubic feet per second.

because of caving banks opposite each other, in straight reaches of the river.

Everything indicates that the Mississippi river is not, and obviously it cannot be, an exception to the law which governs the flow of water in all sedimentary rivers, be they great or small ; that as the normal maximum quantity of water is increased the velocity increases, and, hence, the area of channel-way becomes enlarged and the slopes of bed and surface diminished. The system of leveeing, therefore, as applied to such a river, is based upon correct and sound principles ; the effect of levees, if persevered in and maintained, will be to lower the flood line instead of to raise it, if cut-offs and outlets, which alone interfere with the establishment of a permanent regimen, are prevented.

It has been claimed on high authority,* that the clay bed of the Mississippi "resists the action of the strong current like marble," also that "the bed of the Mississippi cannot yield ;" therefore that it, the river, does not accommodate itself to its necessities and no dependence can be placed upon its doing so, in calculating the effects of closing outlets by building levees. That the river does in the course of time wear away its bed, is not, indeed, denied in so many words, but, practically, as the consequence of increasing the quantity of water flowing in the channel by means of levees, the effect of the resulting increase of current in wearing away and enlarging the channel section is disregarded in calculations made by them. The action of water in slowly wearing channels thousands of feet deep, though even the hardest primitive and volcanic rocks, as for instance through the immense canons of the Colorado, is too well known to be questioned. It is also so well known as to make denial useless, that the action of the powerful Mississippi current upon the hard blue clay, whether alluvial or testiary is not essential, which forms its bed, though comparatively slow as respects its action upon other strata, is sufficiently rapid to allow for and keep pace with the increase required for the gradual extensions of the levee system and the closure of outlets.

In the caving bends of the Mississippi, as usual in all rivers, the water is always deepest, therefore, as the newest excavations of the hard blue clay bottom are made in the bends, it cannot be maintained that "the bed of the Mississippi does not and cannot yield." When cut-offs occur, also, the blue clay bed of the river does not prevent the speedy enlargement of the channel to the dimensions required for the quantity of

* Humphreys and Abbot.

water flowing. The enlargement and deepening of the river sections opposite New Orleans, as determined by actual measurement, also establish the fact that the "bed of the Mississippi" does yield. The undermining of the river banks at high water, which always precedes the caving of the banks at a mid stage or at low water, also shows that the river bed does yield.*

It is also claimed that the Mississippi water at flood is "undercharged with sediment," because of the observed fact that below a bend where the banks are caving in, when the river is falling, at a middle or lower stage, it may and does often happen that the river water is more muddy than at a high stage. It may be true that the river water is, in places, more highly charged with sediment, generally, when the river is declining and the banks are caving in, than at the highest flood stage; for though the stronger current prevailing at full flood scours out the bottom and slowly undermines the banks, the pressure of the water helps to support the banks, and they do not, as a rule, fall in until this support is withdrawn, when the river falls. We concede, that there is very little, if any, deposit made in mid-river, in the thread of maximum current, at the full flood stage, but it does not follow that there is none when the river is declining, or when it is most turbid, and the current is reduced from any cause. Bars continually form under the points and in the eddies, and they encroach upon the channel in the bends. Whatever reduces the current at any stage occasions a deposition of sediment.

It is claimed, by the same high authority, because of the assumption that the river water at flood is "undercharged with sediment," that no deposit can occur below a crevasse or other outlet, and that "all authentic records of actual soundings made above and below the sites of large crevasses justify the belief that no deposits have ever occurred in the channels below them in consequence of said crevasses."

All experience and observation show that where the Mississippi river current is checked from any cause, and at any stage, but more especially

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* In 1874, for instance, the maximum horizontal depth of caving at Morganza, below Red river, during that year, was 550 feet; at Point Manoir, opposite Port Hudson, it was 1 100 feet; at Lobdell's, below, it was 460 feet; near Bayou Goula, it was 350 feet; at Landry's, in Ascension Parish, it was 420 feet; in St. Charles Parish, it was 300 feet, in two places; opposite New Orleans, it was 220 feet in one place and 200 feet in another; while cavings of 220 feet, 160 feet and 80 feet occurred between New Orleans and the forts below. Above Red river, in Tensas Parish, at Kempe's, the effect of the Davis cut-off of 1867, was to cause an average caving in of the river bank during the year 1863 to 1873 inclusive, of 1 200 feet per year. In 1874, the caving at Wilson's, same parish, was 2 100 feet. In Concordia Parish, at Marengo, the maximum caving in 1868 was 3 000 feet, and it was 1 400 feet in 1873. All of which indicate a most unmistakable *yielding* of the "clay bed of the Mississippi."

when the river is falling, there a portion of the earthy matter held in suspension is dropped ; and the more heavily charged the water is, the greater is the deposit. In still water all is deposited, and the water becomes clear ; the proportion of sediment deposited depending on the loss of current velocity. The inference that, because the river at a flood stage is sometimes not so turbid as when the banks are caving in at a mid-stage, in the former case, being comparatively "undercharged," and in the latter, certainly overcharged with sediment, there can be no deposit at any stage, seems unwarranted.

We know that, as the river falls, the bars help to throw or direct the current against the banks in the bends to precipitate or hasten their caving in (at flood, the water flows over the bars in mid-river, mainly), and observation shows that very much of the earthy material excavated in each caving bend is deposited on the next bar or bars below; particularly the coarser, gravelly or sandy particles. A large outlet certainly reduces the mean velocity of the current below it, in the river, very considerably, and, therefore, a larger portion than usual, of the material excavated from the caving bends next above, is and must necessarily be deposited on the bars next below the outlet. Hence the contraction of the channel area below is proportioned to the dimensions of the outlet above and to the stage of the river. All bars increase in proportion to the excavation of earthy material above and the loss of current below; a falling river, or outlet, or both together, add to them, while a rising river wears them away and moves the material composing them further down stream.

Were it not for this "undercharged with sediment" theory, and what are considerd to be the unfounded assumptions based thereupon, that no deposits can occur below an outlet at any stage of the river, which is, in effect, saying that none occur even when the river water is overcharged; it would be unnecessary to discuss this question at such length, but, inasmuch as the outlet and levee systems are irreconcilable and as opposite as the antipodes, and cannot exist together as parts of one system (for outlets inundate and levees reclaim land, and it is claimed by the writer that levees alone are needed, and that they can be fully relied upon) some examples of the effect of outlets upon sedimentary rivers in Louisiana will be given.

During the flood of 1874, at the top of the rise, a crevasse, caused by a muskrat burrow, occurred in the Bonnet Carré Levee, left bank of the Mississippi river, 40 miles above New Orleans, on April 11th. The

highest rise, opposite New Orleans, be it observed, occurred April 15th and 16th, several days later, during a storm, with the wind from the south; but the highest point reached by the river, in 1874, at a point 5 miles above the crevasse, was, as before stated, 6 inches *below* the 1862 flood height. At New Orleans, the tidal or storm rise was, for two days, 8 inches *above* 1862, five days after the crevasse occurred. The Bonnet Carré crevasse became 1 370 feet wide, with a washed out channel in it 550 feet wide, 50 feet deep at high water, and extending one-fourth of a mile back of the levee, where the land was 15 feet below the river flood-line. On July 15th, when the river had fallen 15 feet, the river water ceased to run through the crevasse channel. On September 20th, 21st and 22d, when the river was very nearly at its lowest stage, 20 feet below high water, the writer measured and carefully sounded two river sections above this crevasse outlet, one opposite, and two below it; taking transit angles to each sounding. Sections 1 and 2 were taken 1¾ miles above the outlet, respectively; sections 4 and 5 were taken 750 and 1 500 feet below it. Sections 1 and 2 gave the following dimensions, respectively: widths, low-water, 2 886 and 3 014 feet; maximum depths, 110 and 79 feet; mean depths, 64 and 54 feet; sectional areas, 184 653 and 164 167 square feet; high water widths, 3 120 and 3 210. Sections 4 and 5 gave the following dimensions, respectively: widths, low water, 2 406 and 2 452 feet; maximum depths, 62 and 64 feet; mean depths, 40 and 42.3 feet; sectional areas, 96 640 and 106 150 square feet; high water widths, 3 300 and 3 430 feet. The average dimensions of the two upper and the two lower sections, were as follows: low water widths, 2 950 and 2 429 feet; depths, 59 and 41.65 feet; sectional areas, 174 410 and 101 395 square feet. Reduction of channel below average of upper and lower sections in width, at low water, 521 feet; in depth, averages of whole sections, or mean depth, 17.35 feet; in area of channel section, 73 015 square feet. The mean high water sectional area above was 232 003 square feet, and below, 156 913 square feet; reduction below, 75 090 square feet.

It is not claimed that the whole of the contraction of channel below was due to the crevasse outlet of 1874 (for a part of it is due to the Bonnet Carré crevasse of 1871), but that much of it was, is certain, for the following reasons: the bottom, or river bed, where the upper sections were taken, consisted of firm blue clay, into which the 11 pound sounding lead sunk from 1 to 2 inches only, all the way across; while the depths were tolerably uniform; all indicating a channel of full natural

dimensions, free from deposits. The bottom, where both the lower sections were taken, was a soft, oozy mud or silt, into which the sounding lead sunk from 1 to 2 feet, except near the left bank (where the current was strongest), on the bend side of the river, where the bottom was also firm clay and free from deposits. Opposite the lower sections, the sand bar, on the right bank side, had encroached upon the channel so much that the low water width had been reduced more than 500 feet and a new sandy ridge, evidently a deposit made during the preceding high water, from 12 to 15 feet high at its lower end, had been formed above the low water line and over the old bar, in a direction parallel with the thread of the current from the river above into the outlet opening. Very extensive new deposits of sand had been made in the right bank bend next below, several feet high above low water, and extending several hundred feet out from the shore line; these were *known to be new*.

Everything, the new sand bars and sandy deposits, the new oozy mud or silty deposits, extending nearly across the river channel below the outlet, and the very great reduction of channel-way below caused thereby, indicated most unmistakably that this Bonnet Carré crevasse outlet of 1874, did cause a partial filling up of the river below. Probably the deposits were greatest when the river was declining and the banks were caving in above. The sectional area of this crevasse, allowing 2 feet for depression of surface of water in the opening, was about 32 000 square feet or nearly one-sixth of the river section. As it (at the time of writing) remains open, there will be an opportunity, after the high water of 1875, to determine by a re-measurement of the same sections, what changes will have occurred since 1874.

There are many remarkable examples of the effect of outlets to contract the channel below them, on Red river, which also is a turbid stream like the Mississippi. One of them will be described. Tone's Bayou, about 20 miles below Shreveport, right bank, was an insignificant overflow *coulee* twenty-five years ago. It was cleared out, and its enlargement facilitated, partly to relieve Red river as an outlet. In 1872, its sectional area of discharge had become 5 600 square feet, while that of Red river itself, below, had been reduced to 3 500 square feet, with a width less than 200 feet, and a high water depth less than 30 feet. One mile below Shreveport, where all the water of Red river is confined to one channel, the sectional area of channel at high water is about 23 000 square feet, with a maximum depth of about 45 feet, and a width exceed-

ing 500 feet. Below this point the river is depleted by three outlets besides Tone's Bayou, on the west side, and still others on the east side. Above Tone's Bayou, the other outlets have reduced the main river section to 9 000 square feet, a loss of 14 000 square feet, and below, it is still further reduced to 3 500 square feet by actual measurement, or a total loss of 19 500 square feet of river channel section. An official report,* says: "According to testimony, and from comparison of actual surveys, the channel of the river from Shreveport to Tone's Bayou has, during the past fifteen years, been continually enlarging (because outlets on the left bank were being closed above Shreveport), while the channel below Tone's Bayou has remained stationary, or become actually contracted by the encroachment of trees and deposits of earth on the sides. At present, in the highest stages, the water is nearly at the tops of the banks in the river above Tone's Bayou, in the bayou itself, and in the river below; that is to say, at present, nearly the whole capacity of Tone's Bayou and the river below is required to dispose of the largest freshets from above." Fully five-sixths of the water of Red river, which, now, since the closure of outlets above, all passes Shreveport, escapes to the right and left into the lake basins east and west, through outlets, and yet the flood line in the river below Tone's Bayou is not at all reduced. In fact, if we had the comparative levels of twenty-five years ago for reference, it would probably be found that the flood line below is higher now than then.

The same laws govern in all sedimentary rivers, whether small or great. The first effect of an outlet is to lower the flood line of a river, because time is required for re-adjustment of the river's regimen; but the ultimate effect will be the reverse, because the law is that the less the quantity of water flowing the greater is the slope required for its discharge at a given velocity.

Nowhere else can there be a more striking example of the evil effects of outlets than on Red river; for the lakes above and below Shreveport owe their origin to the great elevations of the bed and banks of the main river opposite them caused by rafts and lateral outlets. The only remedy there, is the gradual and systematic reversal of the process, the building of levees and the closure of outlets from below upwards, thereby cutting off the flow of water into the lakes, and the drainage of the lakes at their down-river ends by means of deep dredging-machine canals excavated into them.

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* By Lieut. Woodruff of the U. S. Engineer Corps.

The Bayou Lafourche—the only original channel of the Mississippi, or outlet, left open below Red river—is another example of the evil effects of outlets, and of leveeing a stream from its head downwards. Its levees do not extend to the Gulf, and outlets and crevasses are the rule yearly on the lower Lafourche. These cause deposits in and contraction of the lower portion of the channel and the raising of the opposite banks by deposits thereupon. The result is the backing up of the water above, and the elevation of the flood line yearly. Where levees of 2 or 3 feet formerly sufficed 50 or 60 miles below the head of the bayou, embankments of 10 feet height or more are now required, while the levee heights at the head of the bayou remain the same as at first; and yet each year before the main river reaches its full height, a break in the lower Lafourche levees must occur, or the water must flow over them. The only remedy is the contraction of the bayou at its head and an extension of the levees to the mouth of the bayou, with a dredging out of the lower contracted channel, if this bayou is to be left open. It would be better to close it altogether and substitute a slackwater or locked canal navigation, for outlets have ruined it and are of no benefit to the Mississippi river.

In calculating the effects of adding to the quantity of water in the main river by closing outlets, or of reducing the quantity by means of outlets, it will not do to assume, as has been done, that the sectional area of the channel below can neither be enlarged or contracted, that it is fixed and unchangeable. That certain determinate and determinable relations exist between the quantity of water flowing, the mean velocity of current, the sectional area of channel-way and the slopes of bed and surface cannot be ignored or disregarded. They must be admitted if a reliable result is desired.

Gen. J. G. Barnard in July, 1850,* propounded the true doctrine, and gave an example, with assumed data, of the true method of calculation. He admitted that "an immediate lowering of surface might be expected from such an outlet," as he assumed at Bonnet Carré, but he demonstrated that "the ultimate effect would certainly be to raise that surface." He said:—"It is pretty well established that certain relations exist between the configuration of the bed of a stream and the velocity of its current. This relation is the more clearly discernable and capable of being subjected to calculation in rivers whose beds have been formed of materials brought down by their own currents, in other words which have made and shaped their own beds." "If from any cause, such as throwing

* DeBow's Review, New Orleans.

off a portion of the water through a water-weir, the velocity of the current is diminished, it is no longer able to maintain its sediment in suspension, but will continue to deposit it in its channel, until through the elevation of its bed, its velocity again becomes what it was before it was disturbed, sufficient to maintain its sediment in permanent suspension. Now it is a well established principle in hydro-dynamics that the less the volume of water, the greater the surface slope required in order to maintain a given velocity."

It is certain that all sedimentary rivers adapt themselves to every change in their regimen ; the Mississippi is no exception, nothwithstanding that its vast magnitude makes even slight changes in it, a work of time. Its flood can be controled by means of a levee system, but only the national government is able to perfect and maintain such. Individuals, at first, constructed and maintained their own levees, but as the effects of cut-offs gradually added to the cost of levee construction and maintenance, by causing the caving in of the higher front lands, the people of a district under took the larger works in the caving bends. Next, the State of Louisiana undertook the work of levee construction by taxing her whole people, It is now found that even the State is powerless to perfect the system. In 1874-5 at least 4 000 000 cubic yards of new levees were needed in Louisiana alone, whereas about 1 000 000 was the limit of the quantity the State could pay for. This amount and more have been built in 1875, but many great gaps still remain open, including two below Red river, which together require half a million cubic yards of earthen embankments to close them.*

As before stated, levees can be relied upon, and levees alone can be. Cut-offs should be prevented wherever possible. Outlets are worse than useless, even if it were possible, which it is not, to provide a free channel to the sea for the water so drawn off ; they overflow land and we wish to reclaim land. Reservoirs are impracticable, and what natural ones there are, only add to the river floods by feeding the rise below. As to the diversion of tributaries, it would be useless even if practicable.

* The State of Louisiana since 1865 has provided for the construction of about 19 600 000 cubic yards of levee, but now 1 000 000 cubic yards, costing $500 000 per annum is all that it can provide for, notwithstanding that such is not nearly enough. The United States Levee Commission of 1874-5, estimated approximately that 114 774 000 cubic yards of levees, at a cost of $45 909 600, would be required to perfect a permanent levee system for the Mississippi valley.

From October, 1866, to October, 1874, 53 miles of Louisiana levees above the mouth of Red river—distance 278 miles—were lost by the caving of the banks of the Mississippi river. Below Red river—distance leveed on both sides 497 miles—about 55 miles were lost from the same cause. The extent of the caving is very much greater and more rapid above than below. Below the Palmyra Bend cut-off of 1867—below Vicksburg—the bank caved in one place 2 100 feet in one year, and at an average of 1 200 feet per year for five years, in another place.

Levees can most certainly be relied upon, and the object of this paper has been to demonstrate that levees alone are needed; that the only way to reduce the flood line and lessen the liability to inundations is to perfect the system. By means of an adequate levee system—which should include the construction, prompt repair and guarding of all levees—and, afterwards, of interior drainage, every acre of alluvial land in the entire Mississippi valley may be reclaimed, cultivated, and the valley made the home of millions of prosperous inhabitants. The way to safety is to reclaim, to populate and cultivate the valley.

Gen. Abbot (United States Engineer) says: "The total area of the bottom lands is about 32 000 square miles, of which a mere narrow strip along the main stream and its principal tributaries and bayous has been heretofore open to cultivation. Protected against the river and properly drained, this would render available at least 2 500 000 acres of sugar land or more than double the amount heretofore planted; about 7 000 000 acres of the best cotton land in the world, capable of yielding a bale to the acre, and not less than 1 000 000 acres of corn land of unsurpassed and inexhaustable fertility. An expenditure of about $3 to the acre of land actually made cultivable by the levees would thus be sufficient to reclaim them from overflow. Supposing the cotton lands alone to be under cultivation, a tax of one cent a pound for one crop, would nearly pay the cost of the levees for the entire valley."*

The United States is engaged in a struggle for the maintenance of her supremacy as the greatest cotton producer in the world, and the only way to maintain this supremacy is to perfect the Mississippi river levee system, and so bring all of the valley lands into cultivation. Surely, the permanent reclamation of the great Mississippi valley, with its ten or twelve millions of acres of the richest alluvial lands in the world, is or should be of sufficient national importance to justify its being undertaken by the general government.

* The total lengths of main levees required to protect the Mississippi valley lands are very nearly as follows:—in Louisiana, below Red river, 497 miles; above Red river, 278 miles; in Mississippi, 380 miles; in Arkansas, 545 miles; in Missouri, 77 miles; making a grand total of 1 777 miles; in Louisiana, interior rivers, bayous and old river lakes require about 925 miles more of levees, making a total for the State of Louisiana of about 1 700 miles.

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CXXII.

APPLICATION OF

THE THEORY OF CONTINUOUS GIRDERS

TO ECONOMY IN BRIDGE BUILDING.

A Paper by CHARLES BENDER, C. E., Member of the Society.

PRESENTED FEBRUARY 23D, 1876.

Does the application of the theory of continuous girders actually lead to economy in bridge building?

Lately, the introduction in this country of continuous girders has been suggested on the plea of greater economy than, it is asserted, can be obtained under application of the highly perfected and simple American trusses. The majority of advocates of that system who are now devoting their time to theory, though well acquainted with the mathematical part of the subject, have yet not acquired that practical judgment which would be necessary to enforce their assertions. In reality, mathematical investigations of the subject of continuous girders require not a very high degree of training in analysis. It needs but the execution of the integration of one single equation, which execution may become lengthy and tedious, and may require much patience. But once the one mathematical idea of that equation be understood, the rest of the work is common and mechanical algebraic labor.

If it were true that continuous girders give more economy than the system in use in the United States, it would certainly be a heavy charge against the engineers who have made the art of bridge building their specialty, and who study their profession with all earnestness. Many will deny, at the outset, that this charge is just; for the sake of others, I

propose to show that the American practice of bridge building hitherto, has been in the proper direction towards further improvements, and that the young theorists who wish bridge builders to follow their advice have studied the subject in but one of its bearings, and have omitted to examine closely their premises as well as their conclusions.

I believe it is not only desirable but *necessary* that this question should be fully discussed, from various reasons. Practical engineers generally do not place much confidence in long formulæ, and if they once have studied mathematics thoroughly they lose the taste for these studies after some time of practice, since they have convinced themselves as to the futility of ultra refined theoretical speculations. These engineers will not be very likely to adopt structures whose calculation of strains would waste so much valuable time. But these engineers could not prevent a new method of construction in time becoming fashionable, whether correct or not, as long as it were founded on some elegant theory and seemingly led to economy. For, under our large factors of safety, we can commit many sins in construction before they are found out. Again, there is always a number of men who, because they do not understand abstruse calculations and formulæ, rather than admit this fact, publicly endorse them warmly. And finally, when in polytechnic schools for a number of years, a certain theory has been thoroughly studied with zealous assiduity, a little army of its admirers will fill positions in railroad and in public engineering offices, anxiously waiting for the first opportunity towards introducing into practice what they consider the finest jewel of their technical knowledge. I frankly admit once to have been of this number. But after studying the subject of continuity of trusses for several years, and a careful examination of its suppositions, I found myself compelled to admit that the theory is not correct scientifically, and does not agree with the physical laws of elasticity of iron.*

I am now prepared to prove, that for medium spans, say of 200 feet, the construction on the principle of continuity leads to *greater* truss-

* Six years ago, in a paper written for the German Society of Engineers (*Verein Deutscher Ingenieure*) in Berlin, which was translated into English, and published two years ago in the *Railroad Gazette* of New York, I stated :

"The writer of these lines himself had for some time thought that it might be possible, by application of pin joints, by reducing the number of parts, by the use of proper scales and adjustments for the regulation of the pressures on the three or more piers of a continuous bridge, and by the use of scientifically correct and complete formulæ, to produce reliable continuous trusses, by means of which the large rivers of this country could be spanned without the use of false works."

"With a great deal of labor he had constructed an analytical expression, which embraced the relation of the moments of flexure over three consecutive piers of a continuous girder. In this formula, due attention was given not only to the deflections caused by the chords, but also

weights in addition to greater cost of workmanship than are required by the use of single spans with improved details.

This last result is very important indeed, for if it were possible, under application of the principle of continuity to arrive at an economy in weight and cost, there would be a large market for this article however objectionable the mode of construction; for, railroad officers in the majority of instances will be led by the consideration of first cost; especially since, in bridge building (thanks to our factor of safety) many errors remain unpunished for a long time, continuous girders with their delusive theory and deceptive stiffness under application of lattice and rivets would gain a wide market.

The theory of continuous girders, as given in text-books, does not permit the philosophy of the principle involved to be clearly seen; its representation generally is rather obscure. In order to explain this principle as clearly as possible, I have worked out a new method of treating the subject. The results under this treatment naturally must agree with those derived from the application of the general theory of the elastic line which, in the last century (1747), was first given by Leonard Euler of Basel, then member of the Academy of Science in Berlin, which, by Navier, early in this century, was propagated among engineers and lately was somewhat simplified by Henry Bertot in France.

I.—The general Principle involved in the Theory of continuous Girders.—We first consider a number of single spans of the lengths l_1, l_2, l_3, l_4, touching each other respectively over the piers B, C, D, E. We suppose each span to be loaded in any conceivable or desired manner; in consequence, each span would deflect so as to form certain curves as indicated by dotted lines. The lower chord would not remain straight, the end-posts would not remain vertical. Differing with the nature of

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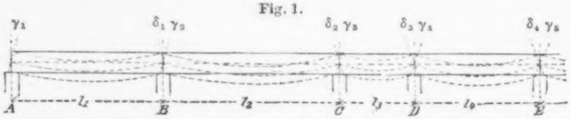
to those due to the tensile and compressive members of the web system; also the actual section of each separate member was introduced. It therefore did away with two errors of the formulæ generally quoted in books, which are only applicable when the girders are very shallow and when the web is a plate, and which even under those suppositions do *not* coincide very satisfactorily with experiments."

"Notwithstanding the theoretical improvements mentioned, it was finally found that the labor spent in finding said formula had been in vain, from a reason which in Europe, as far as known, has not received any consideration. It is the great variability of the modulus of elasticity, which in the formulæ of the books is supposed to be a constant value of about 25 000 000 pounds per square inch."

"But the writer has tested, during his presence at the Phœnix Iron Works, many thousands of eye-bars, made for actual use in bridges, and found that the modulus of these members is very changeable, namely from 18 000 000 to over 40 000 000 pounds per square inch, so that small sections give the lowest and large sections the greatest figures. The same result was obtained by the Canadian engineer who inspected the iron for the International bridge near Buffalo, as well as by Mr. B. Nicholson, who was sent to Phœnixville by the government officers of the United States to inspect the iron for the Mississippi bridge at Rock Island."

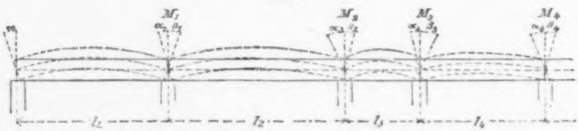
the material, with the sectional areas of the members of the bridge, with the loads imposed upon them, the trusses would show certain angles $\gamma_1, \delta_1, \gamma_2, \delta_2, \gamma_3, \delta_3$, &c., &c., of the end-posts with their originally vertical positions. Now, suppose you draw together the top points of the end-posts over the piers B, C, D, E, and press apart the bottom

Fig. 1.



joints of these posts, so that not only the top but also the bottom chords of the adjacent spans would touch each other; or in other words, insert certain tensile forces into the upper chord, and equally large compressive forces into the lower chord, of each truss. Thus each truss by a certain unknown moment of flexure would artificially be bent upward in such a manner that certain angles $\alpha_1, \beta_1, \alpha_2, \beta_2, \alpha_3, \beta_3$, would be produced, were the dead and the live loads of the trusses removed.

Fig. 2.



When at each central pier, the desired continuity, consisting of connection of the top and bottom chord ends, separated under the dead and live loads alone, but overlapping each other under the action of the moments M_1, M_2, M_3, &c., is effected, this law, must obtain, namely: the sum of the angles of deflection at any central pier caused by the (dead and live) loads on two adjacent trusses must be equal to the sum of the angles of elevation, caused by the unknown moments artificially applied at the three piers of the contemplated spans. This is expressed algebraically:

$$\left.\begin{array}{c}\delta_1 + \gamma_2 = \alpha_2 + \beta_1;\ \delta_2 + \gamma_3 = \alpha_3 + \beta_2 \\ \delta_3 + \gamma_4 = \alpha_4 + \beta_3, \text{ \&c., \&c.}\end{array}\right\} (I.)$$

In these equations, the left members are functions of the dead and live loads, and the right of the unknown moments M_1, M_2, M_3, &c.

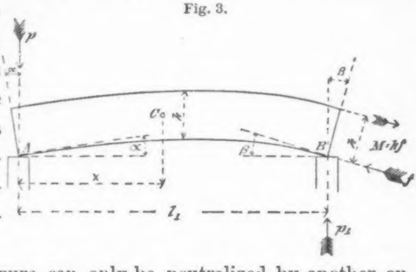
For each intermediate pier there is one equation and one unknown moment. The number of equations equals the number of unknown moments, which equals the number of spans of the continuous bridge less one. For n spans there are $(n—1)$ equations and $(n—1)$ unknown

moments. These (n—1) equations are therefore sufficient to solve the problem which is identical with finding the unknown moments. From the theory of single spans, which again is founded only on the law of the lever, we calculate angles of deflection like δ, γ, α, β, and therefore the above law, expressed by the very plain equations (I), indicates how to derive the strains of continuous girders from those of single spans. This law, as it were, is the tune for all the rest of variations relating to continuous girders.

If we express the angles α, and β, by the unknown moments acting on two continuous spans we arrive directly at the formula of Henry Bertot, improperly ascribed to Clapeyron, which is found after a tedious process of integration. Bertot's formula in fact expresses only the geometrical law that the sum of the angles of deflection must be equal to the sum of elevations due to the moments M_1, M_2, M_3, &c., over the central piers.

Though we suppose the reader to be acquainted with the theory of single span bridges, in the sequel we shall develop a few rules belonging to this theory sufficient for us to directly write down the formulæ from which we calculate the moments M_1, M_2, &c., and consequently the strains. Before doing this we first wish to more fully explain the next consequences of the principle of continuous girders. A span $A\,B$; without weight is resting on two supports A and B; at B a moment M_1, equal to forces $f, -f$, with the lever h acts on the chords. This moment is counteracted by a force (weight) p, holding the truss-end A to the pier. Though the force p, holds down the end A, yet the moment $M_1 = f\,h$ will cause a convex elastic curve, so that the end-posts which originally were vertical are made to form angles α and β, to their vertical positions.

Fig. 3.



Since a moment of flexure can only be neutralized by another opposite moment, there must exist another force,—p_1, acting on the pier B, which in combination with $+p_1$, on the lever l, equals precisely $M_1 = f\,h$; in other words, p_1 must be equal to $\frac{M}{l_1}$ and $M_1 = p_1 l_1$.

For any section C, of the beam $A\,B$, the moment of flexure is equal to the force p_1 multiplied by the distance x, and the greater x is, the

greater the moment of flexure in C. When x becomes equal to l_1 the maximum of the moment is reached, namely $M_1 = p_1\ l_1$, whilst at A, the moment of flexure acting on the beam is zero because x is zero. And as the curvature of a beam increases directly as its moment, the beam is not bent at all at A, but is gradually bent more and more the nearer we come to B.

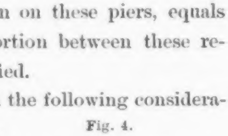
From what has been said, this law can be deduced: that the application of a moment M_1 to the central pier of an end truss span of a continuous girder does call forth two forces, $+p_1, -p_1$, which, however, do *not* alter the *sum* of the reactions A and B of this span in whatever manner it may be loaded.

The moment M_1 reduces the pressure on the pier A, but only by increasing with the same amount,—p_1 the pressure on the pier B. From this observation it further follows that by the principle of continuity, no load resting on an end span can be carried over to the next span, but that the sum of these loads always is neutralized by the two nearest piers between which it acts. The distribution only of the reactions, which for single spans is governed by the law of the lever, in the end spans of continuous girders is modified.

What has been said of a single span acted upon by one moment M_1 is equally true, if on its other end another moment M_2 would act. All we need do is to add together the effects due to each separate moment. Therefore, also, any load acting on a middle span of a continuous girder is taken up by the two nearest piers. Also, in this instance, the sum of the two partial reactions belonging to this span on these piers, equals the total load between them. Only the proportion between these reactions, by the principle of continuity, is modified.

This result could have been anticipated from the following consideration: The strut $C\,D$, Fig. 4, carries down to the pier D the loads due to the span G. As long as the bearing D is inelastic, the diagonal $D\,E$ can not be drawn down and the vertical pressures carried by the members $C\,D$ and $E\,D$ must be directly annihilated in D. The case would be very much different if the pier D were elastic,* for then there would arise a

Fig. 4.

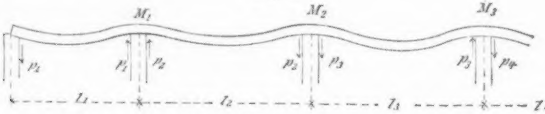


* The supposition of elastic supports, consisting of systems of springs, was investigated by the writer, in an extensive series of calculations, with a view to determine whether thereby any economy could be secured. The result was, that the variable positions of the movable load so much reduced any gain in the chords that the additional expense of the systems of springs left no economy for a structure of this kind.

deflection of the truss $G\,F$ in D, and a portion of the shearing force from one truss could travel to the next one.

Having learned that the moments M_1, M_2, M_3 cause the existence of pairs of forces $+p_1 - p_1$, $+p_2 - p_2$, $+p_3 - p_3$, $+p_4 - p_4$, &c., it is very easy now to express the exact value of the moments by the forces p_1 p_2, &c., and their levers l_1 l_2 l_3, &c. These values simply are (n being the number of spans):

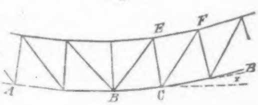
Fig. 5.



$$\left.\begin{aligned} M_1 &= p_1\,l_1. \quad M_2 = M_1 - p_2\,l_2 = p_1\,l_1 - p_2\,l_2. \\ M_3 &= M_2 + p_3\,l_3 = p_1\,l_1 - p_2\,l_2 + p_3\,l_3. \\ M_4 &= M_3 - p_4\,l_4 = p_1\,l_1 - p_2\,l_2 + p_3\,l_3 - p_4\,l_4, \text{ \&c., \&c.} \\ M_{n-1} &= M_{n-2} + p_{n-1}\,l_{n-1} = p_1\,l_1 - p_2\,l_2 + \ldots\ldots + p_{n-1}\,l_{n-1}, \end{aligned}\right\} (II.)$$

and, finally, $O = M_n =$ sum of moments $(pl) = \Sigma\,(pl)$, (from p_1 to p_n.) The last moment M_{n-1} is equal to $\Sigma_{n-1}\,(pl)$ and also is equal to the moment $p_n\,l_n$, so that the $(n-1)$ moments are sufficient to calculate the n forces $p_1\,p_2\;\ldots\;p_n$. After these preparations we proceed to the values of the angles $\alpha_1\,\beta_1\,\gamma_1\,\delta_1$; $\alpha_2\,\beta_2\,\gamma_2\,\delta_2$; &c., &c. Can these values really be calculated? With the answer of this question in the affirmative or negative stands or falls the whole theory of continuity of girders; and this question is followed by another: can we calculate with a sufficient degree of reliability the elastic line of a single span bridge? The originally straight truss $A\,B$ through the influence of live or dead loads has received a deflection. The angle $A\,C\,B$, which originally equalled 180°, has been altered, and the alteration $x\,C\,B$ was caused by the loads.

Fig. 6.



The question then arises, can the angle $x\,C\,B$ be calculated with a sufficient degree of reliability? The alteration of the angles around C is equal to the sum of the alterations of the separate angles. Each angle is altered because each side of each triangle has been altered in length. Some sides have been shortened under pressure, some have been extended under tension. These extensions and compressions are very small, and it is very difficult to measure them.

If we knew the alterations of the sides, also what are the altered angles, calculation would be possible. But to determine the extension or compression of each side of each triangle, it is necessary to know for each

side, the total strain and the exact value of the cross-section, and precisely how much each side (each member) will extend or compress under the action of a ton per square inch. The extension or compression of a member of a girder therefore depends on the strain per square inch at any point of this member, and on quality of the material.

For plain, single span bridges with hinged joints, the law of the lever teaches us to calculate the total strain in any member. We have reduced, in principle, the problem of continuous girders to a combination of problems on single spans. Hence also for continuous girders we could determine those total strains, if it were possible to calculate the deflections of single span girders. But we do not know beforehand the sectional areas of the different members of a continuous girder; on the contrary, it is just our problem to find those sections which are most suitable.

The theory of continuous girders as treated in text-books, leaps over this difficulty by making an arbitrary supposition, namely, that all sections are equal. We shall have occasion to show that this is by no means correct, but causes an error of about 15 per cent. of the calculated values, and this error probably is as large as the theoretical gain claimed for the chords of continuous girders.

As far as the nature of the material is concerned, we know that extensions and compressions are proportional to the strains per square unit, and that, in order to find them, the strain per square unit must be divided by a coefficient proper to the material, which is called the modulus, and the quotient thus obtained must be multiplied by the original length of the member. The next question is, therefore, do we know the value of this modulus for the material used ?

This question must be answered by experiment. It belongs to the science of natural philosophy. The physical suppositions upon which mathematical investigations are based should always be founded on undeniable facts, since the truth of these suppositions is the *conditio sine qua non* of the value of the resulting formulæ. For the supposition being reduced to a mere hypothesis, it is wholly indifferent by what brilliant and elegant analytical or graphical method the deficiencies of the foundations are hidden.

The theory assumes that the modulus of elasticity for any part of a girder is a constant value unaltered to any noticeable extent by manufacture ; or by rivets, covering plates, different sections of rolled iron, or by thickness of the metal, &c., &c. Practical men will be likely to demand that the truth of this broad hypothesis be demonstrated. Therefore, previous to calculating, let us examine the hypothesis of the theory

of continuity. Nowhere, in applied science, the necessity for such examination is more urgent.

The extensions and compressions allowed in practice are very small quantities. Defective testing machines, unacquaintance or unfitness of experimenters to such delicate work, temperature, variability of manufacture of material under test, variability of its chemical composition, density, uniformity, &c., are causes of great errors. Often the number of tests were too small to draw therefrom any justified conclusion, or the elements of time and motion have been neglected, or experimenters overlooked other important elements altogether. There are also instances that experimenters were not impartial, and that theories have been formed first, and experiments have been arranged afterward to suit. Experimenters may reject results which in their opinion seem untrustworthy, whilst they report and elaborate others which to them seem probable because favorable to their theory. Or, experiments were made on one sort of material and applied to material of quite a different nature and section. It is by no means an easy labor to conduct trustworthy experiments on the elasticity of material, and Professor Wullner* is perfectly correct in saying: "The examination of the elasticity of solid bodies is one of the most difficult in the whole science of natural philosophy. In order to conceive its laws sufficiently, the most intricate mathematical investigations and the most subtle experiments are required." These two conditions rarely are found combined.

We shall soon see that the theory of continuous girders was built up exclusively by men of purely mathematical capacities, and that they did not begin with that cautious examination of their suppositions which is demanded by true science as much as by practice. It is true that some theories have been worked out before the basis properly was investigated, and are applicable because the results of such theories were *finally* tested. But we have no such experiments on continuous girders. For the measured deflections of executed continuous bridges are neither sufficiently exact nor are they of use for our purpose, since we do not know what permanent sets were produced after removal of the false-work, nor was the modulus of each member previously examined and recorded.

This remark also applies to single spans, and the question therefore again most pertinently arises, whether the exactly calculated deflection of a single span ever did agree with its real deflection. Logic compels us to acknowledge that such coincidence is impossible, save by sheer accident; for, who has examined the modulus of each finished riveted compression

* In the first volume of his work on physics.

member, or of each rod or bar of a bridge? Who, therefore, was capable of calculating the deflections? Moreover, these calculations generally have been faulty,* and if engineers assert that the deflections of their bridges agree with their calculations, either the first were erroneously calculated or the second not well observed.

Since we have no experiments on the qualities of finished continuous girders, the more should we examine the results of experiments on the values of moduli of iron and steel. These experiments, fortunately, are very numerous indeed, and gives us all information wanted, whilst, unfortunately, theorists on continuity have not considered it worth while to consult this great quantity of scientific material previous to entering into mathematical speculations.

II. Experiments on the Values of the Moduli of Iron and Steel. —Bornet, in France, over 40 years ago, made experiments on rods of chain iron 0.2 inches in diameter and 21 feet long. The original modulus found was 35 500 000 pounds, and under strains of 20 000 pounds per square inch, it decreased to 28 500 000 pounds. Ardant, in France, for soft annealed wire, up to strains of 35 000 pounds per square inch, found the modulus to be 24 000 000 pounds, and for hand-drawn wire, up to strains of 42 000 pounds per square inch, it was 27 300 000 pounds. Whilst of wire, Ardant did not perceive any lowering of the modulus up to strains of 42 000 pounds, Bornet remarked a diminution beginning with strains of 8 500 pounds per square inch. It is not stated by Morin, who reports these results, in what manner the experiments were made.

Hodgkinson made (only) two tensile experiments on long rods to determine their extensions with exactness, and found the original modulus of one bar equal to 23 900 000, and of the other to 22 400 000 pounds; Edward Clark gives 29 000 000 pounds. Vicat's experiments on hard wire with 0.1 inches diameter, resulted in a modulus of 28 200 000, and for wire well annealed, 20 660 000 pounds. Morin for hardened wire gives 28 100 000, and for annealed wire 22 400 000 pounds.

Experiments of a more practical value were made by Malberg, in Prussia, on occasion of his building the Muhlheim suspension bridge. The bars for this structure were made by Herr Daelen. The iron was of

* The influence of the webs on deflections, generally is neglected. With the exception of by Schwedler (see official Engineering Periodical—*Zeitschrift fur Bauwesen, Berlin*), no successful attempt was made to examine the influence of web posts and diagonals. In the Appendix, we shall show that this influence is enormous and very perplexing; in fact, that calculations of continuity of bridges without properly considering the webs, are worse than worthless. The calculations there shown were first developed by the writer in 1869, and are not found elsewhere.

best German stock, the puddle loops well hammered, rolled, piled and re-rolled. All bars were of the same stock, same make, same length, same sectional shape and area. Their moduli, however, varied from 20 000 000 to 27 000 000 pounds, which gives a difference of 35 per cent. for the same kind and section of bars.* This great variability of moduli of bars of even the same shape and material, was further noticed on occasion of the construction of the Vienna Railroad suspension bridge, where bars of the same modulus were put into the same panels.

I have myself had occasion to test many thousand of eyebars, up to about 40 feet length, and varying in section from 1 to 14.25 inches square. The moduli of these bars varied much according to their cross-sections, and were from 18 000 000 to 40 000 000 pounds, and even higher. These results were confirmed by other inspectors of bridge work, for instance, by Mr. B. Nicholson.†

We turn to moduli of steel. Morin, for steel 0.167 inches square, from Petin & Gaudet, found 31 000 000 and 31 800 000 pounds. Direct tensile experiments on Krupp's steel, by Woehler, gave on the average, a modulus of 32 560 000 pounds, whilst from flexure he calculates 31 100-000 pounds.

Prof. Staudinger, of Munich, has made careful tests on Bessemer metal,‡ when the moduli were found to be independent of the quantity of carbon combined with the iron.§ The following table contains the results of his experiments:

Moduli in Million Pounds per Square Inch.

Carbon—per cent.......	0.14	0.19	0.46	0.51	0.54	0.55	0.57	0.66	0.78	0.80	0.87	0.96
Tensile moduli, short pieces test...........	32	30.4	32	31.4	30.6	31.5	31	32.4	32.5	30.5	31	31.2
Compressive moduli....	38.2	37	32.7	32.4	36	33.4	32	35.6	32.4	32.3	31.5	32.7
Tensile moduli of bars...	32.2	.....	.....	.....	33.4	....	32	32.7	.....	.....	.....	.....
Do. screw rods 13 ft. long	28.8	.....	.....	.....	29.5	.....	28.9	31.5	.....	.....	.....	.....
Moduli by flexure........	28.4	29.1	28.4	30	28.8	30.3	29.3	32.1	30.4	33	30.7	29.3
Tortional moduli........	.....	12.15	12.1	.....	12.1	.....	11.9	12.3	12.1	12.7	12.1	11.4

This shows differences of moduli as large as 33 per cent., for the same class of metal. It also proves that Hodgkinson is wrong in quoting the compressional modulus of wrought iron lower than the tensile

* Herr Daclen is an authority, known by his universal mill, a treatise on the art of shape rolling, and his invention of weldless rolled eyebars, known as Howard's Patent. † At Phœnixville, Pa. ‡ From the Pernitz Works in Austria. § The quantity of carbon rose from 0.14 to 0.96 of one per cent. Metal with 0.14 is soft iron, with 0.19 to 0.30 it is granular iron (of a fine grain) or hard iron, then comes soft steel, which increases in hardness with the carbon contained.

modulus; it again gives evidence that the softest metal (that contains 0.14 per cent. of carbon) may give a higher compressional modulus than even the hardest steel of this table.

At the Vienna Exposition, a set of test-pieces could be seen,* which showed as follows:

Carbon—per cent.	1.	0.75	0.5	0.28	0.12	
Specific gravity.	7.83	7.84	7.85	7.86	7.88	
Tensile modulus (millions pounds)	25.1	24.6	27.7	24.9	26.1	
Ultimate tensile strength.	90 000	80 000	70 000	67 000	65 000	pounds.

The modulus of this class of steel and iron was, in the average, noticably lower than those for Ternitz iron and steel, the difference being about 20 per cent.

B. Baker made some experiments on steel bars previous to his experiments on crippling strength and found the modulus from 29 100 000 to 37 330 000 pounds, which result shows a difference of 28 per cent. He says† "Every practical man who has noted the behavior of iron girders under bending stresses, knows whilst one girder may deflect a certain amount under the test, another one precisely similar and placed apparently under precisely the same condition, may deflect some 30 per cent. more or less."

Hodgkinson made two direct experiments on the compression moduli of iron, and found 19 200 000 and 21 000 000 pounds. These two experiments strengthened Hodgkinson in his belief of the correctness of his theory as to a weakness of wrought iron under crushing stresses, whilst they only prove how easily an experimenter may be misled. Duleau, in France, directly measured the compressions of fibres in comparison with their extensions, which he, differing from Hodgkinson, found to be exactly equal.

Very valuable hints as to the qualities of iron can be derived from experiments on flexure, which can be conducted easily with sufficient accuracy. Morin, by such, determined the following moduli:

For iron from works near Rouen	31 800 000	pounds.
" " " Jackson Pétin & Gaudet	28 400 000	"
" " " Ale' Lik (Algeria)	28 960 000	"
" English crown bars	23 440 000	"
" French I beams with equal flanges	29 330 000	"
" " " " unequal "	24 400 000	"
" beams also from Dupont & Dreyfussin Ars sur Moselle,	26 000 000	"
" " " " " equal flanges	23 600 000	"
" " " " " unequal flanges	23 000 000	"
" " " " " same beam reversed	23 000 000	"

Here again we have differences of moduli amounting to 39 per cent., and for the same class of iron (Lorraine beams) of 27 per cent.

* Bessemer metal from the Reschitza Works in Hungary.

† In his book on Beams and Columns.

Thomas D. Lovett* has lately furnished an elaborate series of experiments on compression members, such as actually used in the bridges of the Cincinnati Southern Ry. Up to the time of his report † 65 compression members had been tested and broken ; their moduli varied from 19 300 000 to 34 600 000 pounds.‡

Experiments on hollow wrought iron tubes made by Hosking gave these results :

Modulus of a rectangular tube	20 405 000	pounds.
" " round tube	24 500 000	"
" " elliptic "	24 300 000	"

Moduli of rails, experiments made by Morin :

Tredegar iron, double headed, maximum modulus	27 730 000	pounds.
Vignole's French rails, average modulus	26 400 000	"
Dowlais rails, double headed, minimum modulus	21 100 000	"

the greatest difference being 31 per cent. Morin believed that the great variations of moduli (even of rails of same section and make) should be explained by the quality of the iron, and he judges that the better metal should show the higher modulus. But the great variations also of moduli of bars of undoubtedly excellent make and of great uniformity seem to disprove his judgment. He states that he has met with moduli, as low as 17 000 000 pounds, while I have observed 18 000 000 as a minimum.

There seems to exist this law—that the moduli of bars of same section made from double refined iron bars (rolled three times, packeted and welded twice), such as called *best-best*, are more uniform than bars made from best iron, such as were used by Herr Malberg in the Muhlheim suspension bridge. At least, many thousands of bars tested at Phœnixville, Pa., proved to be remarkably uniform in their moduli as long as they were of the same section, whilst the moduli were very variable when bars of different sections were compared. On the other hand, Styffe's experiments, which were made on excellent steel and iron, gave a maximum tensile modulus of 34 584 000, and a minimum of 27 585 000 pounds, which is similar to that of an iron rail from Avon, in Wales.

Morin's experiments on flexure of unhardened steel gave the following results :

	Maximum.	Minimum.
From Pétin & Gaudet, refined	28 800 000	28 100 000
" " " puddled	31 800 000	29 200 000
" " " crucible	32 300 000	29 200 000
Krupp's	32 200 000	28 700 000
" mean of 17 experiments	30 300 000	
English	28 900 000	

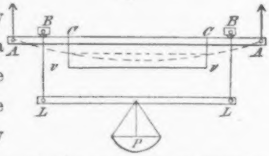
* Consulting Engineer of the Cincinnati Southern Ry. † November 1st, 1875.

‡ These experiments, which conclusively prove the superiority of the American system of bridge details, are very complete, and will, doubtless attract much attention.

It should be noticed, that even for the finest metal that we know, such as crucible steel, the variation in its modulus amounted to 11 per cent., and for the renowned Krupp steel, 12 per cent.

The most accurate experiments are still to be mentioned (those of Herr Woehler), made by suspending the test-piece *A A*, so that it could expand freely; the lever *L L* carried the load *P* and the arms *A B*, *A B* were exactly equal; the piece *B B*, acted upon by a constant moment of flexure, carried the measuring aparatus *C v*, *v C*, by which the deflection of *C C* could be very exactly found.

Fig. 7.



In this apparatus the deflections of the test pieces become comparatively very large, and piece *C C* was free from the influence of knife edges.

These are Herr Woehler's results:

Modulus of iron from Laura huette	28 930 000	pounds
" " Phœnix huette	29 360 000	"
" " Minerva huette	31 680 000	"
" Low Moor iron	31 230 000	"
" homogeneous iron from Pearson, Coleman & Co.	32 340 000	"
" Bochum steel	32 000 000	"
" Krupp steel	31 600 000	"

All these materials were of unusually excellent quality, and the maximum difference still was 12 per cent.

The same variability which we have found to exist between iron and steel, not so much as to the quantity of carbon contained, as from imperfections of manufacture and other causes unknown to us, was noticed with the shearing or torsional moduli. Thus Duleau found for iron from Périgord, moduli of 14 450 000, and again of only of 7 980 000 pounds. Iron from Arrièges gave 8 450 000, English iron 9 860 000, and also 12 800 000 pounds. Wiebe in Berlin quotes the shearing moduli thus:

Soft wrought iron	9 000 000 pounds	Steel	9 000 000 pounds
Bar iron	10 250 000 "	Finest cast steel	14 000 000 "

These figures prove that we cannot know the shearing modulus of any class of steel or iron without direct special experiment.

Our conclusions with reference to the supposition of a constant modulus of elasticity for the calculation of deflections and of continuous girders are—from known and unknown causes:—*First*, plain iron and steel bars vary in their moduli very considerably. The smallest modulus of iron was found to be 17 000 000, the maximum above 40 000 000 pounds. Single refined bars of same stock, manufacture and section vary in their

moduli by 35 per cent. Double refined (*best-best*) bars vary little, as long as bars of same section are tested, but considerably with the sections, the minimum being 18 000 000, and the maximum above 40 000 000 pounds. The moduli of rails vary by 30 per cent., and similar results must be expected from common angles, beams, channels, &c. *Second*, consequently, riveted bridge members composed of angles and plates of various thickness and manufacture, interrupted in their homogeneousness by punched holes, covering, reinforcing splice-plates, &c., must necessarily show still greater variations in their moduli than was found for plain integer bars. *Third*, the hypothesis of a constant modulus of elasticity of the material of a bridge being unfounded, the theory built on such hypothesis should be abandoned.*

Having arrived at such conclusion, we nevertheless must expect to hear an objection against its logical consequences, namely this—that numbers of continuous bridges do good service in practice. So they hitherto have done, not because the principle of continuity is admissible, but because the factor of safety used in their construction has hidden the error made in their design. For the same reason, the Victoria bridge in Canada stands, which is made continuous, but simply by combining two single spans whose greatest chord-sections are in their centres, whilst the greatest chord-strains, according to theory, would fall where the cross-sections are made the smallest. For the same reason, continuous draw bridges stand, which we find composed of two halves, each designed as a single span. I know in one instance, that a Hodgkinson cast iron beam was put in place upside down, so that the heavy tensional flange was under compression while the compressional flange of only one-fifth the area of the tensional one was strained under tension, and yet, on account of the factor of safety, the beam stood.

III.—Other Deficiencies of continuous Girders, as regarding the imperfections of the theory, the danger from defective manufacture, from settling of piers, and the increase of strains by the action of the heat of the sun.

In order to find the exact extension or compression of a member of a bridge, we must know not only the modulus and the total strain of the member, but also its cross-section. The problem of continuous girders,

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* Mr. Baker most pertinently remarks with reference to continuous girders: "The most expert mathematician would have to devote a month or more to the preliminary calculations of a very ordinary bridge, and the result deduced would not after all be more reliable in practice than those arrived at by comparatively simple modes of investigation, chiefly on account of the *varying elasticity of different portions of even the same plate of iron*."

however, is to find this very section. The theory assumes that all sections are equal, or at least that the moment of inertia of a girder or a bridge is a constant throughout. Under this supposition we get *smaller* chord strains over the middle piers than exist in reality.

In the case of two equal continuous spans *under full load*, with uniform moment of inertia, we find the moment of flexure over a middle pier to be equal $0.125\ l^2\ p$, where p represents the total load per lineal foot, and l denotes the length of each span in feet. But if we suppose that the same bridge, under full load, shall be strained equally per square inch, the co-efficient 0.125 becomes 0.1464, which indicates strains over the middle piers 15 per cent. higher. In reality, the continuous bridge being not perfectly varied in chord sections, the difference will be less; but it may be remarked that the chords of a continuous bridge, properly designed according to specification, would only be about 10 per cent. lighter than those of equal single spans. With an enormous amount of labor, this deficiency of the ordinary theory can be corrected, and it has been done in a few bridges. But considering the irregularity of the moduli, such labor seems superfluous.

A serious cause of errors in the construction of continuous girders refers to the distribution of strains over the posts and ties in case that two or more web systems have been adopted. In a single span bridge, a load brought on a panel joint of one separated web system, being split into two shearing forces in accordance with the law of the lever, there cannot be any mistake about the strain in a web member, as long as the end posts are vertical, and if they are inclined, the error can amount to only one increment of one panel load.

The problem of web strains with continuous girders depends not only on the law of the lever, but also on the angles of deflection $\alpha, \beta, \gamma, \delta$—not only of one, but of *all spans together*. We remember that by the moments M_1, M_2, M_3, &c., forces $\pm p_1$, $\pm p_2$, &c., were originated, which disturb the law of the lever. If, therefore, in a continuous bridge there are two or more web systems, we are utterly ignorant as to the distribution of the reactions over the two or more systems which, at every end pier and at every middle pier are connected. How much of p_1, p_2, p_3, &c., is acting in one, and how much into the other system? This we do not and cannot know, for the distribution of the reactions will depend entirely on variations due to manufacture, in the mill as well as in the shops. It may even happen that a member of one system receives tension and the other compression. It is therefore very desirable that continuous girders should be built with but one web system.

Hitherto in all our investigations we have made the supposition that the erection of continuous girders was of such perfection that the single spans were connected under the action of moments M_1 M_2, &c., which accorded completely with a perfect theory. Even with the best staging and under the supposition of the most careful workmanship it will be hard to perfectly fulfill this condition. But suppose it were possible, and that a pier settled. In this instance, the girder would receive considerable disturbances of its strains, which in some points would be decreased, while in others increased. The deeper the girder, the greater the disturbance from this cause would become, so that it seems advisable to leave to the girder as much plasticity as possible, by adopting a depth smaller than demanded by simple theoretical economy. In fact, this change in the values of calculated strains could become enormous; hence the piers of continuous girders should be built more substantially than is necessary for single spans. But this caution is costly.

The real economy of continuous girders, as claimed in Europe, when compared with single span lattice bridges, consisted in building the girders on land and then rolling them over the piers. This mode of erection is elegant, but it does not fully secure the fit of the superstructure to its bearings on the piers, and it is still doubtful whether this mode of erection can compete with that of single spans, designed with the specific American details. In the subsequent example of two 200 feet continuous girders, we shall give figures as to the disturbance of strains in case the girders do not fit their supports.*

What has been said as to the disturbances of strains by settlement of piers or by badly executed girders, is equally true in regard to the effect of the sun. Swing bridges have been drawn crooked by the rays of the sun falling upon one side. In others, the bottom chords are covered by floor timbers, and the top chords are considerably overheated by the sun, or unequally cooled under sharp winds. The effect of this unequal temperature is enormous, and it is sufficient (even) to raise a continuous bridge from a pier. In the case of tubular girders, this objection has peculiar force. Hereafter we shall take examples and calculate the strains caused by change of temperature of the top and bottom chords of continuous girders.

The last objection urged against continuous girders, refers to the mode of proportioning those parts of their chords which at each passage of a

* In 1867, the writer gave attention to the practical solution of the old idea of weighing the reactions of continuous girders, and designed a cheap apparatus to accomplish this purpose. But the plan had previously been tried in the erection of a bridge in Silesia, Prussia.

train have to stand pressure as well as tension. The space for two spans thus strained equals 33 per cent. of the length of each chord. The European practice to proportion these parts is to find the maximum total strain, divide it by the maximum specified strain per square inch, and make the actual section as near to this theoretical section as can be done. This is radically wrong. Herr Woehler's experiments have established beyond doubt, that the strain which controls the durability, equals the sum of the maximum tension and compression of a chord piece. A bar strained tensily to 35 200 pounds per square inch, can stand, say, 100 000 000 repetitions of such strains, but if at the same time strained compressively to 35 000 pounds, it will break after a small number of repetitions, say 100 000, whereas if strained to the limits of $\pm$ 17 600 pounds, it will show as much durability as if tensively strained to 35 200 pounds.*

Writers on continuous girders, generally erred in comparing girders of various systems of the same depth, whereas the proper depth of girders is a measure peculiar to each system of design and essentially depending on the relative quantities of chord and web-strains. The smaller the web-strains, the deeper a girder can be built. But the web material needed for continuous girders exceeds that of single-span girders by, say, 10 per cent., while the material necessary for the chords (according to theory) is just about as much less. The consequence is, that an increase of depth increases the web material more rapidly than is the case for single-span girders. Because of this, probably, parabolic girders were built much deeper in Europe than quadrangular trusses, and there is no reason why this same principle should not be applied to continuous girders. Even an engineer like Prof. Kulmann in Zurich, made the mistake of comparing parabolic, continuous, quadrangular, single and Warren girders by supposing all of them to be of the same depth, namely, one-tenth of their lengths.

For comparison, I here give a few figures taken from the calculations for the new Buda-Pest bridge † in Hungary, now under progress of erection. There will be 4 spans, of 321 feet, carrying two tracks, depth 32 feet, each calculated for 3 000 pounds per foot ; strains 9 740 pounds per square inch ; compression correspondingly ; weight of parabolic trusses 285 500, and of continuous lattice trusses 270 300 kilogrammes (two spans each). We know that American trusses of proper proportions can be built lighter and cheaper than parabolic trusses, and therefore, also, in this in-

* For further information on this subject, compare Herr Woehler's Report in the *Berliner Zeitschrift fur Bauwesen*, from which a few results are quoted in Appendix A.

† See Stummer's Engineer, Vienna, 1875.

stance there is no reason for giving on the score of greater economy the preference to continuous lattice trusses. But the contractor had made a very low bid and desired the continuous bridge to be chosen, though originally, single spans were designed and bid upon. These continuous girders are intended to be rolled over the piers. The depth of one-tenth is decidedly too low for single spans; they should have been taken at 40 instead of 32 feet.

Herr Schwedler, in Berlin, who certainly has as much experience in European bridge building as any other engineer, and who is so much an authority in theoretic matters that not even the most distinguished theorist can very well set him aside, in 1865* had made it his strict rule neither to build nor to recommend continuous girders or arches without at least hinges at the skewbacks. He builds a species of bow-string girders with depths of one-seventh of the span, which, though to American eyes is complicated in details, yet are decidedly superior to continuous girders.

I have now explained why I cannot admit that it is desirable that American bridge engineers should seriously regard continuous girders, however attractive they may be to some mathematicians on account of the wide field for interesting problems presented, and shall proceed to briefly lay down the theory such as derived from the suppositions which were found questionable, whereupon an application shall be made to two 200 feet railway spans in comparison with single spans. There we shall find occasion to test all what has been said in previous paragraphs, also to examine the probable or possible errors of design, and thus to arrive at final conclusions.

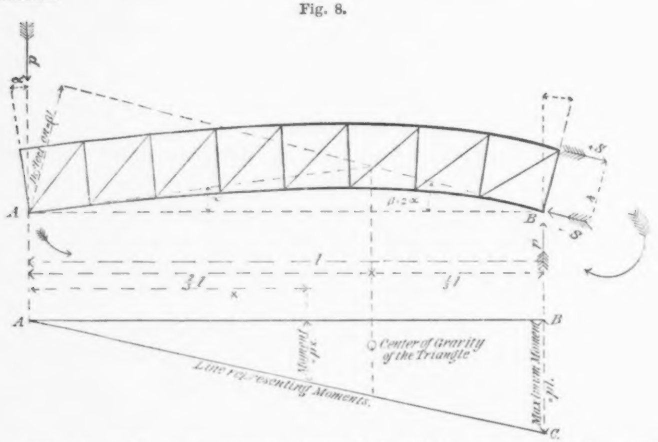
This investigation will have a negative and also a positive value; negative, because we spend useful time on the study of an objectionable system, and positive, because we once more will have occasion to learn that in our art also, the most perfect system must be the most simple one.

IV.—General Development of a simple Method of Finding the principal Formula of continuous Girders.—The angles α, β, δ, γ, &c., can be found by considering one single formula developed in the theory of single span beams, which in the following paragraph will be used repeatedly. For the present, we make the same suppositions which are used by other writers, though contested by us. We assume therefore, *First.* The modulus of elasticity of iron is a constant and known

* See Herr Schwedler's theses on bridge building in the *Zeitschrift fur Bauwesen.* The very excellent scientific pocket-book, *Des Ingenieurs Taschenbuch des Verein Hutte,* 10th edition, Berlin, does not treat continuous bridges.

value. *Second.* The continuous girders throughout have the same cross-section, moreover they have straight parallel chords. *Third.* The deflections of these girders are not modified by the shearing forces; in other words, the struts and ties of the web do not change their lengths. *Fourth.* The web systems of the girders are so arranged that there is no doubt of the office of each separate system of struts and ties, which condition can only be fulfilled in case of but one system of diagonals and posts. *Fifth.* The temperature of all members is alike, and cannot change in any separate member. We use this notation: E is the modulus of elasticity in pounds per square inch, which, as known, is the measure of stiffness of material, the greater the modulus or the less the value of $\frac{1}{E}$ the less proportionally are the elastic deformations. I is the moment of inertia of the girder, equal for a skeleton truss, to the cross-section of one chord multiplied by one-half the square of depth of the truss, all dimensions taken in inches. Like E, the value I stands in inverse geometrical proportion to the deflection of a beam or girder. $p_1\ p_2\ .\ .\ .\ p_{n-1}$ denote the *elastic reactions* in pounds caused by the unknown moments $M_1\ M_2\ M_3\ .\ .\ .\ M_{n-1}$ over the middle piers of continuous girders. $l_1\ l_2\ l_3\ .\ .\ .\ l_n$ are the lengths of the spans in inches, consequently $M_1\ M_2\ .\ .\ .\ M_{n-1}$ must be measured in pound inches.

Fig. 8.



The above figure represents a truss $A\ B$, which is supposed to be acted upon by no other forces but the pair $+S_1, -S_1$, which create a moment $M=Sh$ counteracted by a force p in A. This force p in combination with

$-p$ in B on the lever l ($=A\ B$) has the tendency to turn the truss $A\ B$ in opposite direction to M, and to produce equilibrium; consequently pl must equal M. The sum of the horizontal as well as of the vertical forces being zero, no movement of the truss $A\ B$ will be possible; nevertheless its elasticity will cause a flexure which increases in curvature from A to B. This is due to the moments of flexure increasing in geometrical progression from A to B, which moments in the triangle $A\ B\ C$ are represented by the straight line $A\ C$. The maximum moment occurs at B and is $= M = Sh = pl$. For any distance x, from A the moment will be $Mx = px$.

The above figure also represents that the total strains in the chords increase in geometrical proportion from A to B. At B the total strains will be S and $-S$, in A the strains will be zero. The chords being supposed to be equally strong in section, the strains per square inch likewise increase in a geometrical progression from A to B. The web strains, however, remain constant through the whole girder, because, according to the nature of this problem, the shearing force has a constant value$=p$.*

We know that the expressions for the angles α and β must contain E and I as divisors, and l and the maximum moment as multipliers, so that we only need find the coefficient to this expression. Actually the development gives:

$$\left.\begin{aligned} \alpha &= \frac{1}{6}\frac{M\,l}{E\,I} = \frac{P\,l^2}{6\,E\,I} \\ \text{and} \qquad & \\ \beta &= \frac{1}{3}\frac{M\,l}{E\,I} = \frac{P\,l^2}{3\,E\,I} \end{aligned}\right\} (III.)$$

so that β is twice as great as α; (see Fig. 8).

In case the truss $A\ B$ had been perfectly varied in sections to suit the moments, the coefficient of β would no more be $\frac{1}{3}$ but would have increased to $\frac{1}{2}$, which is 50 per cent. more than under the supposition of a constant moment of inertia I, for any section of the truss $A\ B$; which result indicates need of caution in making this supposition for continuous girders.

The simple law contained in Eq. *(III)* is sufficient to easily solve the remainder of questions embodied in the theory of continuity. Suppose the girder $A\ B$ to be acted upon by this moment M_1 in A and by the moment M_2 in B, both moments acting towards an increase of upward flexure. What will be the angles α and β? This problem is only a

* In Appendix B, it will be shown how the formulæ for the angles α and β, can, under the suppositions made, be found without the aid of the infinitesimal calculus.

corollary to the first. We have: end points A, B; moments at these, M_1, M_2:

Angles due to M_1 at $A = \dfrac{M_1\, l}{3\, E\, I}$ at B $\dfrac{M_1\, l}{6\, E\, I}$

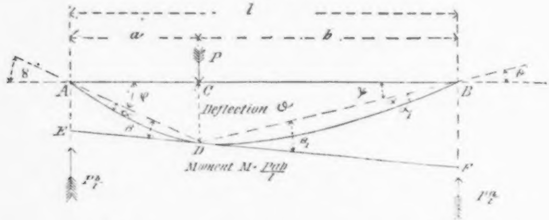
" " M_2 " $\dfrac{M_2\, l}{6\, E\, I}$ " $\dfrac{M_2\, l}{3\, E\, I}$

Total angles......... $\alpha = \dfrac{M_1\, l}{3\, E I} + \dfrac{M_2\, l}{6\, E I}$ $\beta = \dfrac{M_1\, l}{6\, E I} + \dfrac{M_2\, l}{3\, E I}$

In case M_1 were $= M_2$ there would be throughout the girder a constant moment of flexure, and α would become equal to $\beta = \dfrac{M l}{2\, E\, I}$. In this instance, the elastic line would be uniformly curved, and part of a circle whose radius is $\rho = \dfrac{E\, I}{M}$, as well known from the theory of single spans.

Finally, we have to determine the angles γ and δ of a single span exerted by a single panel load P. In Fig. 9, P represents the panel load at the distances a and b from points A and B, $a + b$ being equal to $A\, B = l$. By the law of the lever, the reaction of the pier at A will be

Fig. 9.



$\dfrac{P\, b}{l}$ and at pier B, it will be $\dfrac{P a}{l}$. $E\, F$ representing the tangent on the elastic curve at D, the angles β and β_1 are known as well as the angles α and α_1. The angles $\varphi + \psi$ together must equal $\beta + \beta_1$. (Consider that $\varphi + \psi +$ angle $D = 180^\circ$, and that $\beta + \beta_1 + D$ also $= 180^\circ$). The angles of deflection being very small, can be considered as equal to their tangents, namely: $\varphi = \dfrac{d}{a}$; $\psi = \dfrac{d}{b}$, and $\varphi : \psi = b : a$. But on the other hand $\beta = \dfrac{M\, a}{3\, E\, I}$ and $\beta_1 = \dfrac{M\, b}{3\, E\, I}$ so that $\beta_1 : \beta = b : a$, and since $\varphi + \psi = \beta + \beta_1$ apparently $\beta_1 = \varphi$ and $\beta = \psi$, so that simply:

$$\left.\begin{aligned} \gamma &= \alpha + \psi = \frac{M\, a}{6\, E\, I} + \frac{M\, b}{3\, E\, I} \text{ and } \gamma = \frac{P\, a\, b}{6\, l\, E\, I}\,(a + 2\, b) \\ \gamma &= \alpha_1 + \psi = \frac{M\, b}{6\, E\, I} + \frac{M\, a}{3\, E\, I} = \frac{P\, a\, b}{6\, l\, E\, I}\,(2\, a + b) \end{aligned}\right\} (IV.)$$

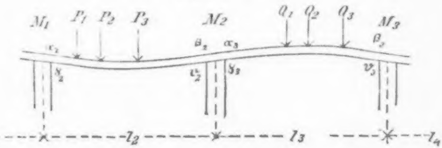
In case the girder $A\,B$ should have carried any number of loads, P_1 P_2 &c., with distances $a_1\,b_1$, $a_2\,b_2$, $a_3\,b_3$, &c., there would have been—

$$\gamma = \frac{1}{6\,l\,E\,I} \times \left[\begin{Bmatrix} \text{the sum of all} \\ \text{expressions} \end{Bmatrix} P\,a\,b\,(a+2\,b) \right] = \frac{\Sigma\,P\,a\,b\,(a+2\,b)}{6\,l\,E\,I}$$

$$\gamma = \frac{1}{6\,l\,E\,I} \times \left[\begin{Bmatrix} \text{the sum of all} \\ \text{expressions} \end{Bmatrix} P\,a\,b\,(b+2\,a) \right] = \frac{\Sigma\,P\,a\,b\,(2\,a+b)}{6\,l\,E\,I}$$

Now we are prepared to write the final formula of continuous girders. The equilibrium of the moments M_1, M_2 and M_3 with the forces P_1, P_2, &c., and Q_1, Q_2, Q_3, is found from (Eq. *I.*) $\delta_2 + \gamma_3 = \beta_2 + \alpha_3$ where $\delta_2 + \gamma_3$ are the angles of deflection due to P_1, P_2, &c., Q_1 Q_2; and $\beta_2 + \alpha_3$ are the angles of elevation due to M_1, M_2 and M_3—of the spans considered as single ones.

Fig. 10.



Their values are : $\delta_2 = \dfrac{1}{6\,E\,I\,l_2}\,\Sigma\,[P\,a\,b\,(2\,a+b)]$ for span l_2,

$$\gamma_3 = \frac{1}{6\,E\,I\,l_3}\,\Sigma\,[Q\,a\,b\,(2\,b+a)] \text{ for span } l_3.$$

$$\beta_2 = \frac{M_1\,l_2}{6\,E\,I} + \frac{M_2\,l_2}{3\,E\,I};\ \alpha_3 = \frac{M_3\,l_3}{6\,E\,I} + \frac{M_2\,l_3}{3\,E\,I};\ \text{consequently}$$

$$6\,E\,I\,(\delta_2 + \gamma_3) = M_1\,l_2 + 2\,M_2\,(l_2 + l_3) + M_3\,l_3.\quad (V.)$$

which actually is the equation of Henry Bertot ; also :—

$$\frac{1}{l_2}\,\Sigma\,[P\,a\,b\,(a+2\,b)] + \frac{1}{l_3}\,\Sigma\,[Q\,a\,b\,(2\,a+b)] =$$
$$M_1\,l_2 + 2\,M_2\,(l_2 + l_3) + M_3\,l_3.\quad (VI.)$$

This equation is of the first degree and contains three unknown quantities, viz.: M_1, M_2, M_3, whilst the expression on the left side is fully known since the loads P_1, P_2, P_3, &c., and Q_1, Q_2, Q_3, with their distances a and b from the end-points of each truss are given quantities.

At every central pier of a continuous girder there is an equation of this form, and there is also an unknown moment, so that we have as many equations of the first degree as there are unknown moments. The problem to find these moments consequently is solved analytically, though the labor of solving these equations algebraically, in case of many spans is rather tedious.

If we introduce for a and b the corresponding number of panels, call $l = n\,d$, where d is the length of each panel, put $a = m\,d$ and $b = l - a = (n - m)\,d$, we arrive at these simplifications for the expressions on the left side of Eq. *(VI);*

instead of $\frac{1}{l}\ \Sigma\ [P\,a\,b\,(a+2\,b)]$, we get $\frac{d^2}{n}\ \Sigma\ [Pm\ (n-m)(2\,n-m)]$ (*A.*)

" " $\frac{1}{l}\ \Sigma\ [Pab\ \ (2a+b)]$, we get $\frac{d^2}{n}\ \Sigma\ [Pm\,(n^2-m^2)]$ (*B.*)

The expressions A and B for the spans $l_1\ l_2\ l_3\ l_4$, &c., l_n being denoted by $A_1\ B_1,\ A_2\ B_2,\ A_3\ B_3,\ A_4\ B_4$, &c., $A_n\ B_n$, Eq's *(VI)*, become : (since the first and last moments M_o and $M_n = O$

$$\left.\begin{array}{c} A_2 + B_1 = 2M_1\ (l_1 + l_2) + M_2\ l_2 \\ A_3 + B_2 = M_1\,l_2 + 2M_2\ (l_2 + l_3) + M_3\ l_3 \\ A_4 + B_3 = M_2\,l_3 + 2M_3\ (l_3 + l_4) + M_4\ l_4 \\ \text{\&c.,} \qquad\qquad \text{\&c.,} \qquad\qquad \text{\&c.} \\ A_{n-1} + B_{n-1} = M_{n-2}\ L_{n-1} + 2\ M_{n-1}\ (l_{n-1} + l_n) \end{array}\right\}\ (VII.)$$

For two continuous spans, there is but one middle pier, and we have this equation only :

$A_2 + B_1 = 2M_1\ (l_1 + l_2)$ and, in case that $l_1 = l_2$, finally

$$M_1 = \frac{A_2 + B_1}{4\,l} = \frac{d}{4\ n_2}\left[\Sigma \left\{\begin{array}{c} Pm\,(n-m)\,(2\,n-m) \\ \text{for span II.} \end{array}\right\} + \Sigma \left\{\begin{array}{c} P\,m\,(n^2-m^2) \\ \text{for span I.} \end{array}\right\}\right]$$

The values $M_1\ M_2\ M_3$, &c., M_{n-1} being found; Eq's *(II)* teach how to calculate the *elastic reactions* $p_1\ p_2\ p_3$, &c., p_n, which in combination with the static reactions of each span due to the law of the lever give the actual reactions of the piers, that may be positive or negative, compression or tension.

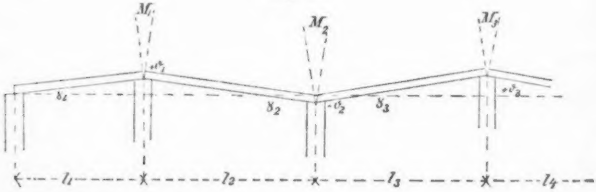
The values E and I in Eqs. *(V)*, *(VI)* and *(VII)*, have totally disappeared, but the suppositions of their being constant throughout the whole bridge *are embodied* in the equations, and without these suppositions being fulfilled the equations cease to be correct. In fact, E and I have only disappeared because we supposed them to be *constant* values, and should not have disappeared in reality.

Next we have to make corrections of these formulæ for the instances that the continuous girder does not properly fit to its bearings, on end or middle piers. Such misfits may arise from settling of the piers, from bad manufacture of the iron trusses, or from the effect of the rays of the sun being greater on one chord than on the other, or from winds cooling one chord sooner than the other. This investigation, which simply consists of a reapplication of the principle of continuity, will give us another opportunity to show how simply these problems can be solved with our method.

Suppose $A\ B$ to be a straight line drawn through the two end bearings of a continuous girder, and that $d_1\ d_2\ d_3$ denote the depressions or

elevations of the middle piers, positive in case of elevation, negative in case of depression. Further, suppose the continuous girder to be cut

Fig. 11.



into n single spans, freely placed on their supports. The problem then is this, which additional or correctional moments M_1 M_2, &c., M_n are necessary to again connect the girders continuously?

This problem at the first sight is nearly the same as that which we have solved. In the previous case, the moments M_1, M_2, M_3, had to lift up the single spans in such a manner as to make the sum of deflections $\delta_m + \gamma_{m+1} = \beta_m + \alpha_{m+1}$. In this problem there are also angles δ and γ; but δ and γ of each span are equal in value, which in the problem just solved was not necessarily the case. Again, the angles δ and γ in the solved problem, were below the horizontal line drawn through the middle pier, whose equation (I) was under examination. In the present problem, those angles may be above that horizontal line, consequently there may be cases when we shall have to consider them as negative.

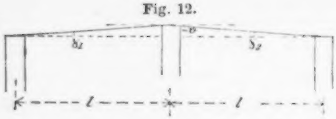
There may arise instances that one or more of these moments will no longer draw together the top chords of the trusses, but push them apart; in other words, the moments M_1 M_2 may bring pressure in top chords, and tension in the bottom chords. We then have for our problem $\delta_m = \gamma_m = \pm \left(\frac{d_m - d_{m-1}}{l_m}\right)$; the positive sign to be taken if the leg of the angle is below and the negative if the leg is above the horizontal line through the middle pier under consideration. In all other respects the problem is the same as the one we have just described; namely, this is the general equation:

$$6\,E\,I\,(\gamma_m + \gamma_{m+1}) = M_{m-1}\,l_m + 2\,M_m\,(l_m + l_{m+1}) + M_{m+1}\,l_{m+1}.$$

For n spans, there are $(n-1)$ equations of this kind, and the moments M_0 and M_n are equal to zero. The values γ are to be substituted with their proper signs.

In the special instance of two equal spans d, being an elevation of the middle pier above the line A, C, we have

Fig. 12.



$\gamma_1 = \gamma_2 = \delta_1 = \delta_2 = \frac{d}{l}$. Both angles are *below* the line $A\ C$, and consequently positive; we therefore have

$$4\,M = \frac{6\,E\,I}{l}\left(2\ \frac{d}{l}\right) \text{ and } M = \frac{3\,E\,I\,d}{l^2}$$

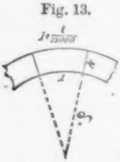
so that this elastic end reaction becomes

$$P = \frac{3\,E\,I\,d}{l^3}. \quad (VIII.)$$

If d had been negative, M would have been a moment, causing (above the middle pier) pressure in top and tension in bottom chord.

We now proceed to our last theoretical problem, namely, to calculate the influence of heat on one chord. Suppose, therefore, a properly manufactured girder resting on supports $A\ B\ C\ D$, etc., with the bottom chord covered by floor-planks; the top chord expands by the heat of the sun, the difference of temperature between both chords being t degrees Fahr. In this instance, the uniformly heated top chord will expand $\frac{1}{150\,000}$ of its length for each degree Fahr. If the girder is unloaded it must assume a flat arc, whose radius is easily found. Two posts which

Fig. 13.



originally were parallel have spread apart $\frac{150\,000}{t}$ of the panel length, and consequently $1 : \rho :: \frac{t}{150\,000} : h$ or $\rho = 150\,000\,\frac{h}{t}$. But the radius ρ being found, it is easy to also calculate the elevation of this flat circle above each middle pier, and this known, the problem is at once reduced to the previous one.

Especially, for two equal continuous spans there is an elevation of the girder equal to $\frac{l^2}{2\rho}$; which is the natural position of the girder considered without gravity, the bed plate being placed $\frac{l^2}{2\rho}$ below the bottom chord; we have therefore $d = -\frac{l^2}{300\,000}\,\frac{t}{h}$ and Eq's *(VIII)* (in inches), $M = -\frac{E\,I}{1\,200\,000}\,\frac{t}{h}$ and $p = -\frac{E\,I}{1\,200\,000}\,\frac{t}{l\,h}$, (IX);
where the minus sign indicates — regarding M, that the moment causes compression in the top, and tension in the bottom chord—and regarding p, that the end piers really are pressed by this elastic reaction; in other words, that p *increases* the pressure on the end piers as caused by dead and live loads on the girder. Dead and live loads, however, actually press down the girder to the middle pier either partly or wholly.

The moments of correction, M_1, M_2, M_3, &c., of a girder being found, for unequal positions of bed-plates as well as under consideration of heat in one of the chords, these results have to be represented on the diagram sheet of moments, shearing forces and strains, and be added algebraically to the moments and shearing forces due to the dead and live loads, when it will be seen whether these last are sufficient or not to cause pressure always on the bed-plates.

Having now laid down the mathematical principles of the theory of continuity, it remains a mere mechanical labor to apply these principles and their resulting formulæ to any practicable number of continuous spans, which labor may, however, require much patience. There is also enough to occupy those few theorists who are not satisfied with a finite number of continuous spans, but wish to extend the theory to the case of any number of openings from unity to infinity.

For the purpose of this paper it will be sufficient to explain the use of these formulæ on two equal continuous railroad spans, by which calculations we shall gain the opportunity to prove numerically the opinions laid down in previous paragraphs.

V.—Calculations of the Strains, Sections and Weights of Two 200 Feet Railroad Spans, compared under the same specification with a 200 feet single span.

Specification.—To construct two 200 feet, square through spans, 14 feet between trusses, of most economical height, with iron cross bearers and with iron stringers, 8 feet apart. For live load consider a train equal to 2 240 pounds per lineal foot, headed by a locomotive concentrating on a cross bearer $1\frac{3}{4}$ tons per lineal foot of a 16 foot panel. For lateral and transverse stiffness assume wind pressure of 25 pounds per square foot acting on the bridge when filled with passenger cars. Maximum direct strain in any point of the bridge to be 10 000 pounds—shearing strain 8 000 pounds—per square inch, and compressional sections of columns with flat ends to be multiplied by the factor $(1 + \frac{n^2}{5\,000})$ where n represents the length of member measured by the least diameter of gyration of a mechanically well-built post; compression members with hinged joints to be treated correspondingly, according to theory. The connections of web diagonals and chords to correspond with the supposition of the calculation.

Under this specification, we divide the 200 feet spans into 12 panels of 16 feet 8 inches long, and we assume the dead weight per lineal foot equal to 1 200 pounds.

First.—Calculations of a continuous bridge of two spans, 200 feet each.

In accordance with the specification of one truss the

panel live load is $\frac{2\,240}{2} \times \frac{200}{12} = 18\,666$ lbs.; increment, $\frac{18\,666}{12} = 1\,555$ lbs;

panel dead load, $\frac{1\,200 \times 200}{24} = 10\,000$ " " $\frac{10\,000}{12} = 833$ "

locomotive excess, $\frac{2}{3} \times 18\,666 = 12\,444$ " " $\frac{1\,244.4}{12} = 1\,037$ "

In the equation for moment over the middle pier (Eq. *VII*) the following are to be substituted, for P, 18 666, 10 000 and 12 444; for n, 12; for m, 1, 2, 3, &c., to 11; for d, $\frac{200}{12} = \frac{100}{6}$ and for l, 200.

The elastic reactions p must be subtracted from the static reactions $P\,\frac{(n-m)}{n}$, and be added to the reactions of the middle pier $\frac{Pm}{n}$. In the table, calculation of the values p, $P\,\frac{n-m}{n}$ and $P\,\frac{m}{n}$ is carried out for a panel dead load, a panel live load, and a panel locomotive excess placed successively on the joints 1, 2, 3, &c., 11, of one span. The combinations of these values for both spans lead to the maxima reactions over end piers (A), and over middle piers (V). They also form all material necessary to calculate the maxima moments, not only of M over the middle pier, but at any other vertical section of the girders.

We will next calculate the maxima moments:

(*a*.) Moments due to dead load. $A = 40\,170$;

$M_1 = 2 \times -7\,415 \times 200 = -2\,966\,000$ pound feet. Any moment M_m, is found by considering the m panel loads acting at their joints. There is namely, in accordance with the law of the lever:

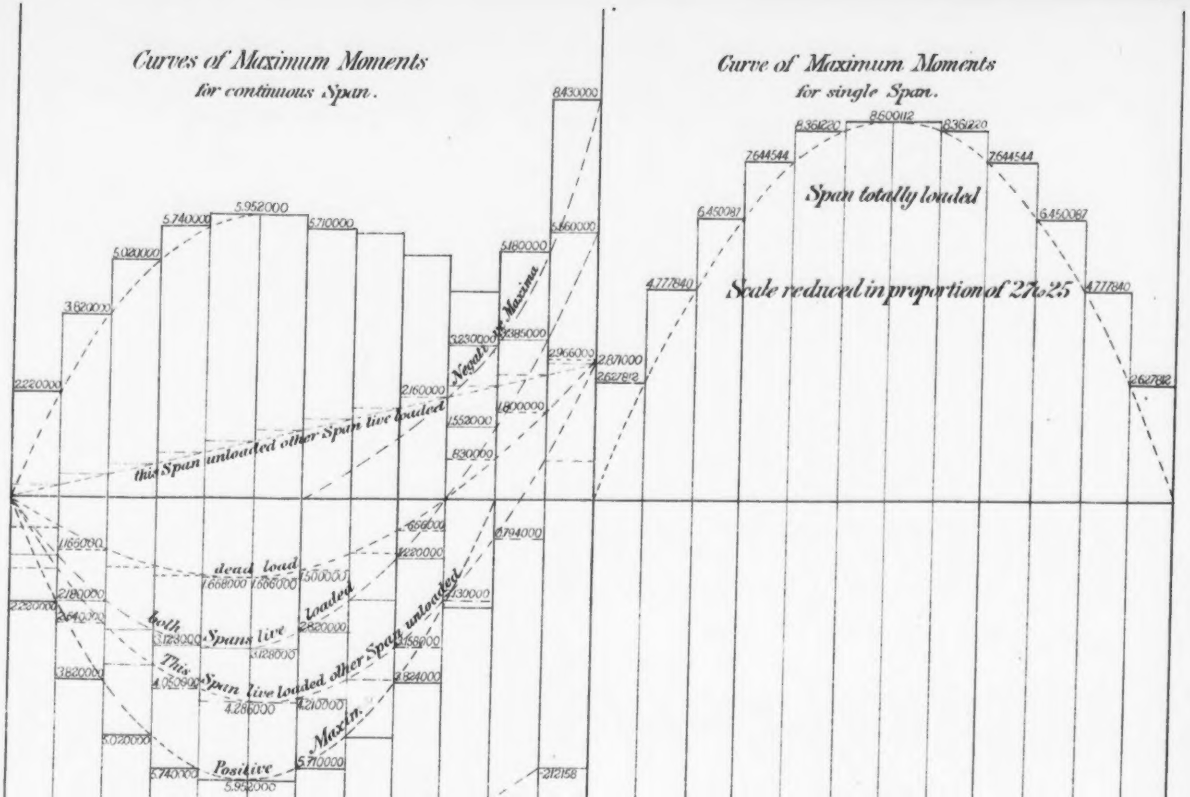
$$M_m = 40\,170 \times m\,d - (1 + 2 + 3 + \&c., + (m-1))\,d \times 1\,0000.$$

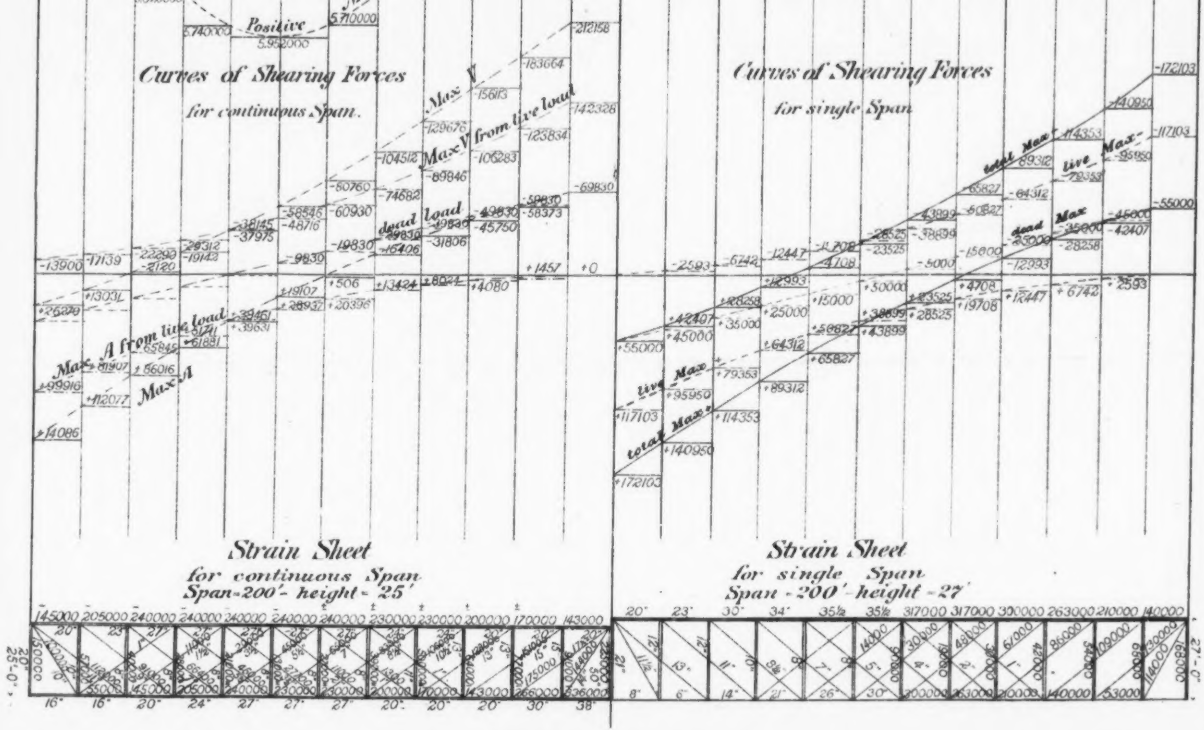
$$\text{or, } M_m = [40\,170 - (m-1)\,5\,000]\,m.\frac{200}{12}$$

The value of this formula can be easily measured on the diagram of forces. (See Plate.)

(*b*.) Curve of moments due to full live loads. Here is $A = 74\,866$, $V = 130\,466$, $p = 27\,800$ pounds, and $M_1 = 200 \times 27\,800 = 5\,560\,000$ pound feet. Any moment $M_m = [74\,866 - (m-1)\,9\,333]\,m.\frac{200}{12}$. (See Plate.)

(*c*.) Curve of moments due to live load on one span only. $A = 88\,766$, $p = 13\,900$, $M_1 = 2\,780\,000$ pound feet. $M_m = [88\,766 - (m-1)\,9\,333]$ $m.\frac{200}{12}$ (See Plate.) The curves thus obtained enable us to construct the maximum moment for any panel of the bridge. This is done on the


Curves of Maximum Moments
for continuous Span.
Curve of Maximum Moments
for single Span.
Span totally loaded
Scale reduced in proportion of 27 to 25
this Span unloaded other Span live loaded
Negative Maxima
dead load
both Spans live loaded
This Span live loaded other Span unloaded
Positive Maxim.
8.430000
5.952000
5.740000
5.710000
5.020000
3.620000
2.220000
5.180000
5.560000
3.230000
3.385000
2.966000
2.871000
2.627812
2.160000
1.800000
1.552000
.830000
8.600112
8.361220
7.644544
6.450087
4.777840
2.627812
1.650000
.666000
1.220000
.794000
1.668000
1.666000
1.500000
2.180000
2.220000
2.640000
2.130000
3.123000
2.820000
3.128000
3.158000
3.820000
4.050000
4.286000
4.210000
3.824000
5.020000
5.740000
5.710000
5.952000
.212158



AM. PHOTO-LITHOGRAPHIC CO. N.Y. (OSBORNE'S PROCESS.)

Panel Number $= m$.	REACTIONS.												Locomotive Excess. $=31\,111-18\,666$ $=12\,444$.	
	Panel Dead Load = 10 000 Pounds.						Panel Live Load = 18 666 Pounds.							
	A.			V.			A.			V.				
	Static Load. $P\frac{n-m}{n}$	$-p$.	A. Shearing. *Up.*	Static Load. $P\frac{m}{n}$	$+p$.	V. Shearing. *Down.*	Static Load. $P\frac{n-m}{n}$	$-p$.	A.	Static Load. $P\frac{m}{n}$	$+p$.	V.	A.	V.
1	9 166	206	8 960	833	206	1 039	17 111	386	16 725	1 555	386	1 941	11 150	1 294
2	8 333	403	7 930	1 666	403	2 069	15 555	756	14 799	3 111	756	3 867	9 866	2 578
3	7 500	583	6 917	2 500	583	3 083	14 000	1 094	12 906	4 666	1 094	5 760	8 604	3 840
4	6 666	737	5 929	3 333	737	4 070	12 444	1 382	11 062	6 222	1 382	7 604	7 375	5 069
5	5 834	859	4 975	4 166	859	5 025	10 888	1 607	9 281	7 777	1 607	9 384	6 187	6 256
6	5 000	933	4 067	5 000	933	5 933	9 333	1 749	7 584	9 333	1 749	11 082	5 056	7 388
7	4 166	958	3 208	5 834	958	6 792	7 777	1 796	5 981	10 888	1 796	12 684	3 987	8 456
8	3 333	921	2 412	6 666	921	7 587	6 222	1 728	4 494	12 444	1 728	14 172	2 996	9 448
9	2 500	816	1 684	7 500	816	8 316	4 666	1 531	3 135	14 000	1 531	15 531	2 090	10 354
10	1 666	634	1 032	8 333	634	8 967	3 111	1 188	1 923	15 555	1 188	16 743	1 282	11 162
11	833	364	469	9 166	364	9 530	1 555	683	872	17 111	683	17 794	581	11 862
Sums.	55 000	−7 415	47 585	55 000	+7415	62 415	102 666	−13 900	88 766	102 666	+13 900	116 566	....	....
Reactions one span loaded A.			47 585	....	$V=$	62 415	....	$A=$	88 766	....	$V=$	116 566	....	....
Other span also loaded.....			−7 415	....	....	+7 415	....	....	−13 900	....		+13 900	....	....
		$A=$	40 170	....	$V=$	69 830	....	$A=$	74 866	....	$V=$	130 466	....	....

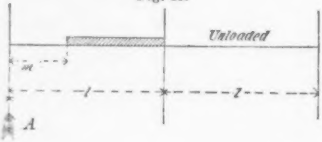
diagram by adding the positive and negative moments occurring at any points. The moments M_a, M_b, M_c, can also be obtained by calculating or drawing, first, the curve of moments under the consideration of single spans, and then the (straight) line of moments due to the elastic reaction p, whereupon the difference of these values, for any point m, agrees with the values calculated as above.

The maxima shearing forces are now to be calculated :

(*a.*) Shearing forces due to the dead load. These forces can be easily obtained by subtracting 1, 2, 3, &c. $(n-1)$, panel dead loads from the value $A = 40\ 170$. This operation is represented on the diagram of shearing forces.

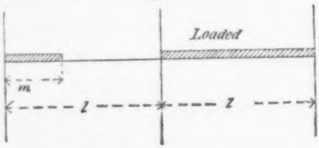
(*b.*) Maxima live shearing forces acting in the direction of A. For this purpose, the second span is supposed to be unloaded, and a train to extend from the panel m to the middle pier.

Fig. 14.



(*c.*) Maxima live shearing forces acting upward in the center. For the calculation of these forces the second span is supposed to be loaded, and the first span also loaded from the end pier to panel m.

Fig. 15.



The bridge of which we are calculating the forces is shown with two web systems. It has been explained * why it is impossible to calculate exactly, the strains occurring in each one of these systems. It must now be added that this uncertainty also necessarily attends the chord strains, whose determination is especially difficult in those chord pieces which, at each passage of a train, have to bear compression as well as tension. There is no method to overcome this imperfection of the theory. In the following calculation we have separated the systems, and have supposed that each system would act independently.

For the calculation of the forces V_c, there arises another difficulty. The value p namely, is influenced considerably by the total load on the second span. Two methods are possible, either to combine the system, 1, 3, 5, &c., 11 of the first span, with the system 1, 3, 5, &c., 11 of the other span, or with the system 2, 4, 6, &c., 10 of this second span. We have assumed, that instead of $\frac{13\ 900}{2} = 6\ 950$, somewhat more, namely, 8 000

* Page 162.

pounds would be the value of p (due to the second span) for each of the systems of the partly loaded span, Fig. 14.

The following tables exhibit the results of the calculation, which is made simply by adding the respective values taken from the preceding table.

	Panel.	Dead Load.		Live Load.		Excess of Locomotive.		Shearing Maxima.	
		A.	V.	A.	V.	A.	V.	A.	V.
System 0—1—3—5—7—9—11—12.	0—1	8 960	1 039	16 725	1 941	11 150	1 294	82 556	....
	1—3	6 917	3 083	12 906	5 760	8 604	3 840	53 275	....
	3—5	4 975	5 025	9 281	9 384	6 187	6 256	27 962	17 033
	5—7	3 208	6 792	5 981	12 684	3 987	8 456	6 481	38 833
	7—9	1 684	8 316	3 135	15 531	2 090	10 354	....	63 717
	9—11	469	9 530	872	17 794	581	11 862	....	91 146
	11—12	....	....	....	....	....	....	....	120 448
		26 213	33 785	48 900	63 094	32 600	42 062	....	....
System 0—2—4—6—8—10—12.	0—2	7 930	2 069	14 799	3 867	9 866	2 578	67 391	....
	2—4	5 929	4 070	11 062	7 604	7 375	5 069	40 101	6 778
	4—6	4 067	5 933	7 584	11 082	5 056	7 388	16 723	26 873
	6—8	2 412	7 587	4 494	14 172	2 996	9 448	....	50 274
	8—10	1 032	8 967	1 923	16 743	1 282	11 162	....	76 506
	10—12	....	....	....	....	....	....	....	104 965
		21 370	28 626	39 862	53 468	26 575	35 645	....	....

The shearing maxima A and V are now to be combined, so as to obtain the total maxima in each panel, such as represented on the diagram of forces. For the calculation of the diagonal and post strains, the two systems again must be treated separately.

The diagrams for the chord and web strains in combination with the two tables referring to the web strains of each separate system, can now be used to calculate in the usual manner the members of the proposed bridge. For this purpose we choose a height of 25 feet (one-eighth of the span) and after consideration of the web strains in the end panels we arrive at the diagram of strains represented. An examination of these strains will give proof that the chord strains cannot be properly determined without consideration of the diagonals, and that consequently the mere theoretical

comparison of curves of moments and shearing forces may lead to considerable errors.

Top and bottom chords of continuous girders after all, can not be calculated with perfect certainty, even under the objectionable suppositions made. It is, therefore, advisable to construct them to resist tension as well as pressure. A proper section for these chords would be, two built channels connected with lattice bars at top and bottom. The pins can be put with mechanical correctness through the centre lines of these channels, and the re-enforcements can be placed so that the pins bear against the metal added to the web plates. We construct the diagonals from weldless eyebars, and the counter rods with swivels. This arrangement has a scientific advantage. Each web member carries but one kind of strain; whereas, in bridges with diagonal web members, only diagonals, at least near the centre of the span, have to resist tension as well as pressure, and therefore must be designed to sustain the sum of both. Moreover, vertical posts are more convenient, with reference to the construction of the joints.

The built chord channels are calculated to be 16 inches deep, and the angle bars 3×3 inches, the latticing to be double top and bottom. The posts are also designed on this basis, with 2 rolled channels and latticing; their bearings are made flat against the bottom and top chords; the radii of gyration have been duly calculated.

The strains upon compression members are in exact agreement with the formula, the factor $\left(1 \times \frac{n^2}{5\,000}\right)$ increasing from 1.12 of chords to 1.82 of posts; the section of the lightest post is taken at 8 square inches.

On this basis the sections of chords, diagonals, posts, counters, &c., have been determined in agreement with the specification. From the strain sheet thus obtained (see Plate) this bill of materials is calculated:

Chords, latticing, joint and reinforcing plates	85 912	pounds.
Posts with latticing, top and bottom bearings, rivets	35 560	"
Diagonals and swivels	43 292	"
Pins and rollers with cages	3 850	"
Cross-beams, hangers and washer-plates	16 000	"
Stringers	25 600	"
Struts and portals	6 000	"
Lateral rods	4 000	"
Castings (end post and head, bed-plates, &c.)	5 000	"
Floor bolts and washers	3 000	"
Total weight of one iron 200 feet span	228 214	"

Iron, per lineal foot	1 141	pounds.
Timber and rails	300	"
Total dead load, per foot	1 441	"
Assumed weight per foot	1 200	"

A too light dead load, therefore, was assumed; but this error amounts to less than 5 per cent. on the truss weights proper, say about 5 000 pounds in the span. The corrected weight of the iron-work of this continuous bridge would then amount to 1 166 pounds per lineal foot.

For the sake of comparison under precisely the same specification, for the same form of truss and the same number of panels, a 200 feet single span has been calculated. This is the strain sheet with data and mode of computation:

Span 200 feet; 12 panels, 16 feet 8 inches long; diagonals for 27 feet depth, $31\frac{3}{4}$ and 43 feet; secants, 1.17 and 1.59; tangents, 0.61 and 1.23; dead load, $1\,200 \times \frac{200}{24} = 10\,000$; live load, $2\,240 \times \frac{200}{24} = 18\,666$ pounds per panel; excess of locomotive load on a joint, 12 444 pounds.

MAXIMA SHEARING FORCES.

SYSTEM.	1.	3.	5.	7.	9.	11.	12.
From dead load..............	30 000	20 000	10 000	....	—10 000	—20 000	—30 000
" live "	....	—1 555	—6 222	—14 000	—25 000	—38 888	—56 000
" locomotive excess.........	....	—1 037	—3 111	—5 185	—7 259	—9 333	—11 400
Maxima..........................	....	....	....	19 185	42 259	68 221	97 400
Diagonals........................	....	....	....	30 000	67 000	109 000	114 000

SYSTEM.	2.	4.	6.	8.	10.	12.
From dead load.........................	25 000	15 000	5 000	—5 000	—15 000	—25 000
" live "	....	—3 111	—9 333	—18 666	—31 110	—46 000
" locomotive	....	—2 074	—4 148	—6 222	—8 296	—10 370
Maxima..................................	....	....	8 481	29 888	54 406	81 370
Diagonals................................	....	....	14 000	48 000	86 000	130 000

Chords.—$3 \times 0.61 \times 28\,666 = 53\,000$; $2 \times 1.23 \times 28\,666 = 70\,600$;

$1 \times 1.23 \times 28\,666 = 35\,300$; $2\frac{1}{2} \times 1.23 \times 28\,666 = 87\,000$;

$1\frac{1}{2} \times 1.23 \times 28\,666 = 53\,000$; $\frac{1}{2} \times 1.23 \times 28\,600 = 17\,600$.

Addition.—53 000, 87 000, 70 600, 53 000, 35 300, 17 600; whence the chord strains, 53 000, 140 000, 210 600, 263 600, 299 000, 317 000 pounds.

The compression members are designed—first, as done with the continuous span; and second, to consist of hollow segment columns. The same sections are adopted for both cases, but with the hollow posts we gain all latticing and still have a greater factor of safety in regard to ultimate strength.

Bill of Materials.

Top chords	49 500 pounds.	Hollow column chords	39 000 pounds.
Bottom chords	28 000 "	Bottom chords	28 000 "
Pins and rollers	4 500 "	Pins and rollers	5 000 "
Posts	34 000 "	Hollow posts	27 300 "
Diagonals	37 000 "	Diagonals	37 000 "
Cross bearers, &c	16 000 "	Cross bearers	16 000 "
Stringers	25 600 "	Stringers	25 600 "
Lateral struts and portals	6 000 "	Lateral struts and portals	6 000 "
Lateral rods	4 000 "	Lateral rods	4 000 "
Castings	5 000 "	Castings	12 400 "
Floor bolts	3 000 "	Floor bolts	3 000 "
Total weight of iron	212 600 "	Total weight of iron	203 300 "
Weight per foot	1 063 "	Weight per foot	1 017 "
Timber and rails	300 "	Timber and rails	300 "
Total weight per foot	1 363 "	Total weight per foot	1 317 "
Weight assumed	1 200 "	Weight assumed	1 200

The weight assumed, 1 200 pounds, consequently was too light also for a single span, and the truss weight should be increased by 4 per cent., so that the actual weights would be respectively 1383 and 1337 pounds per foot. These still are respectively 58 and 104 pounds less than we obtained for the continuous girders.

Having now seen that in the construction of two continuous spans there is no economy, if compared with properly designed single spans, it will be well to examine the weights in detail.

These following, are the percentages of weights as calculated for a supposed dead load of 1 200 pounds per foot.

	Continuous Girders, 25 feet deep.	Single Spans 27 Feet Deep.	
		Latticed Posts.	Hollow Columns.
Chords	37.7	36.4	33.
Webs	34.6	33.3	31.6
Both	72.3	69.7	64.6
Balance	27.8	30.3	35.4

This comparison shows that the more perfect the detail design, the smaller the percentage of weight taken up by the chords and webs. The single span, with hollow wrought iron segment columns, gives the best result. The single span, with latticed posts, is superior to the continuous girders with latticed posts and chords, for the chords and webs still contain 2.6 per cent. less of the total weight in the first design than in the second. This is not alone due to the height, 27 feet, of the single span, for an increase in height would hardly reduce the chords of the continuous girder, since $\frac{5}{12}$ of these chords cannot be reduced in section without lowering the heights of chord members, and therewith reducing the admissible chord pressure.

The panels' lengths being taken at 16 feet 9 inches, the truss height could be increased to 33 feet without losing weight in diagonals, but the posts would become considerably heavier.

The maximum height of a truss is reached when an increase in height causes an increase of total weight; that is, when by an increase of height the web and lateral bracing increases more than the chords decrease.

The most perfect compressional members permit the use of the greatest depth, since the weight of the posts is a large part of the total weights The single span, with hollow wrought iron segment posts (Phœnix columns), therefore, has the smallest dead weight. From the variability of strains in their chords and webs, continuous girders require continuous riveted chords. Under this construction, loss of material seems unavoidable, because these members cannot be made without it, in practically too small sections at the points where the moments became zero. On the other hand, it will be found advisable in continuous girders to avoid too great a variety of riveted members intermixed with eyebars. This construction has been tried several times in this country with drawbridges, but it is doubtful whether any gain actually is obtained by such design.

The continuous girders, such as here designed, have one advantage over the fixed span, because the posts have been arranged with two flat ends, whereas the single span was designed with posts of but one flat bearing.

The following comparison shows that the web of the best designed single span is 23 per cent. lighter than the web of the continuous girder. Theoretically (compare strain sheets) this advantage of the single span amounted to only 12 per cent.

	Continuous Girder.	Single Span; Latticed Members.	Single Span; Hollow Columns.
Posts, pounds...............	35 560	34 000	27 300
Diagonals, "	43 292	37 000	37 000
	78 852	71 000	64 300
Ratio...............	1.23	1.11	1.00
Chords—Pounds............	86 000	78 000	74 000*
Ratio...............	1.16	1.05	1.00

Theoretically the chords compare thus: continuous girder 4 403, to single span 5 026, or as 1.00 to 1.14. In other words, for the same height of trusses, 27 feet, though the continuous girder appears theoretically to save 14 per cent. in the chords, in reality it causes a loss. While a continuous girder of three spans, proportioned according to theory, would show a gain in the chords of 33 per cent., in fact (see Laisle and Schubler) executed examples of acknowledged excellence of design give only 15 to 20 per cent. and this gain is only comparative since obtained under sacrifice of height, the depth being one-twelfth instead of one-eighth the average length of the three continuous spans.

That curves of moments and shearing forces are not sufficient to base a proper estimate of the value of a structure upon, the bridge at Mainz illustrates. This bridge has spans of 344 feet, is 50 feet deep, and designed for a live load of 1 920 pounds per foot, while the strains in top and bottom chords on Pauli's plan are nearly 12 000 pounds per square inch. The dead load of iron of this bridge is 2 100 pounds per foot, whereas an American 350 feet span with iron cross bearers and stringer floor, 40 feet deep, weighs but 1 800 pounds, the maximum strain being 10 000 pounds per square inch. And yet the bridge at Mainz is one of the lightest designs in Europe. It follows then, that mere theoretical considerations of the nature of moments and shearing forces are by no means sufficient. In fact, we arrive at better judgment by studying the proper proportions of depth, panel length, detail arrangements of compression and tensile members, and especially their connections at the panel joints. It will be seen from what has been here developed that continuous girders can only lead to economy in one instance,

* With castings.

namely, if by reason of an improper specification the use of well proportioned single spans were prohibited. Indeed, improper specifications not unfrequently are given to bridge builders, by which, without necessity, very limited depths of trusses and very short panels are fixed, showing that the engineer who wrote such, did not realize what should be specified and what not.

Hitherto we have developed the strains, sections and weights of two continuous railroad girders of 200 feet length. We have made the incorrect supposition that the moment of inertia of these girders is a constant value. But in reality the effective sections of the parallel chords vary from 12 to 38 square inches. Therefore, the curves of moments and the values of shearing strains given by our formulæ, and on which we based the estimate of weight, are not correct. We have stated that the maximum moment over the middle pier would be 15 per cent. greater, were the moment of inertia of the girders so varied as to produce an equal maximum strain of 10 000 pounds per square inch of the bridge totally loaded, and this difference in continuous girders with three openings sinks to 7 per cent. In the given example, however, under the suppositions made, the difference between actual and calculated maximum moments is less than 15 per cent. What it actually is, can be determined by the use of a similar corrected theory, but would involve great labor, without bringing us much nearer to the actual strains.

This tedious calculation has been made in some instances, as the Vistula bridge, near Dirschau, and the error was taken into account in the design of the Krementschug bridge over the Dnieper in Russia. This bridge was designed in 1866, with all possible economy.* It consists of two parallel and separate structures, one for roadway and one for double railway tracks. There are four through spans, 118 metres or 387 feet between centres, bridged by two pairs of continuous girders. The calculation was based on a dead load of 3 710 and on a live load of 3 600 kilos per meter for each track (respectively 2 480 and 2 400 pounds per foot). The weight of the iron work proper, amounts to 2 340 pounds per lineal foot of each track. There are two trusses, 10.5 meters or 34.5 feet deep, which is a depth $\frac{10}{113}$ of the span. The webs are designed as stiffened lattice work arranged in ten systems, the diagonals running at

* By H. Sternberg, Civil Engineer and Professor in Karlsruhe. Through his kindness, I have received copies of the design, strain sheet and estimate, so that the figures quoted deserve the more confidence, as referring to the completed work of not a mere theorist, but one of much practical experience in matters of design and manufacture. The bridge was, however, built upon another design.

angles of 45°. The diagonals of the meshes are 2 meters or 6.56 feet long, equal to the distance of cross bearers of 4 feet 2 inches depth. The rail-stringers are of wood.

The following weights have been calculated :

Chords of one span of two tracks	946 000	pounds.
Webs " " " " "	508 000	"
Bracing and floor beams, &c.,	360 000	"
	1 814 000	

Or 2 343 pounds per foot of each track. These weights are obtained under maximum strains of 8 500 pounds to the square inch (600 kilos per square centimeter) both for compression and for tension of compressional diagonals and chords as well as of parts under tension, for which latter ones the net areas are considered.

The lightness of webs was much furthered by the diagonal system and by adopting the same strain throughout as far as practicable. This lightness of course is secured, but only under sacrifice of certainty as to the diagonal strains and of the postulate of scientific determination of these strains. Nevertheless, the Krementschug design is certainly one of the best proportioned and most economical *lattice* bridges, and gives evidence of its being designed by an engineer who knows how to appreciate expense of manufacture in the mill as well as in the shops and field. This bridge when compared with other European structures is light, and comparatively much lighter than the Mainz bridge on Pauli's plan, so much the more as the latter is designed for a 20 per cent. lighter live load and for 33 per cent. greater strains. Yet compared with single quadrangular trusses, designed with the most improved American details, the Krementschug bridge does not show economy in material, and in regard to manufacture and erection it cannot compete with single spans. A quadrangular double track truss bridge of proper proportions and details, 400 feet span, can be built with the same weight of iron per lineal foot for the same live load, and the same average maximum strain.

In the calculation of the Krementschug bridge, the moments and shearing forces were first determined according to the usual theory, whereupon the correction was introduced due to the varied moment of inertia of the structure. The maximum moment over the central pier under full load of the bridge amounted to 12 300 000 kilogrammeters, but was corrected to 14 420 000 kilogrammeters. The maximum moment, however towards the middle of each span was only reduced by $2\frac{1}{2}$ per

cent. The chords, therefore, by the correction were increased, as also were the web-strains, slightly.*

VI.—Estimate of possible Irregularities in the Strains of the two continuous 200 feet Railroad Spans investigated in the previous paragraphs.

We will now consider the irregularities caused in the strains of continuous girders if, from any reason, they do not fit to their bedplates. For this purpose, we refer to Fig. 11 and to Eq's *(VIII)*, and assume *first*, that the masonry of the middle pier has settled one inch. We have for the moment of correction and for the correction of the pier reactions $M = \frac{3EId}{l^2}$ and $p = \frac{M}{l}$; E being the modulus, assumed at 30 000 000 pounds, I the average moment of inertia, equal to say 7 200 inches pounds, and l the length of the span in feet, consequently, $M = \frac{3 . 30\,000\,000 . 7\,200 . 1}{12 . 200 . 200} = 1\,350\,000$ pounds feet. This moment of correction will produce pressure in the top chords and tension in the bottom chords over the middle pier. The *reaction* of each end pier will be increased by $\frac{1\,350\,000}{200} = 6\,750$ pounds. The bridge being fully loaded by reason of the settled pier, the moment over this pier will *decrease* from 8 430 000 to 7 080 000, that is, by 16 per cent.

If the bridge were fully loaded only on one span, the maximum moment within this span would *increase* by $p\,\frac{5}{12}\,l = 6\,750 . \frac{5}{12} . 200 = 562\,000$ pounds feet. The maximum moment at the fifth panel (see Plate) was 5 952 000 pounds feet, and increases to 6 514 000 pounds feet. This is an *increase* of $9\frac{1}{2}$ per cent., or about as much as it was expected to save in the chords under application of the theory of continuity.

If from any reason—defective construction in the shape, or the middle pier being built too high, or the end piers having settled—the bedplate on the middle pier should lie comparatively too high by *one* inch, the central maximum moment would be increased by 1 350 000 pounds feet,

* The design of Herr Sternberg's bridge has this advantage over many newly built lattice bridges, that the lengths of compressional members are so chosen as to permit their being proportioned for crushing and not for crippling. The chords, 40 inches wide and 32 inches deep, are made only 6 feet long between panel joints, are braced laterally every 6 feet in the bottom chords (cross-bearers), and every 12 feet on top, so that the top and bottom lines of each chord are held in position. The compressional diagonals form diaphragms between the vertical chord plates, 32 inches deep; for panels of 13 and 15 feet, the chords must be proportioned against crippling, their radii of gyration must be correctly calculated and inserted in the formula. Thin vertical plates of channel-shaped chords should be secured in position by diaphragms, and the lateral bracing should be properly calculated and proportioned. The radius of gyration for channel-shaped chords of lattice bridges is very small, and the chord sections will increase considerably, if the specification is duly enforced.

and the total strains over the middle pier would be increased by 44 000 pounds, or by 16 per cent. of their calculated values, and the moments within the span would increase or decrease correspondingly. For every other inch of difference between bedplate and girder bearing the same correctional strains would arise, for Eq's *(VIII)* teach that the corrections are proportional to the values of elevation or depression.

It follows then, conclusively, that the introduction of continuous girders requires the best class of foundation and masonry for the piers. Alone from this reason, practical engineers would not like to use delicate superstructures like continuous girders, even if these would afford some economy of material, which, as we have seen, is not the case.

It was proposed, long ago, to improve continuous girders by weighing the reactions. But the question arises, whether this improvement, which involves some additional cost, is any longer necessary when we know that the theory is of so little practical value. It is also questionable whether a continuous girder, regulated by scales for one mode of loading, would still be properly adjusted under any other position of a moving train; for we know that with each other position of the load, other diagonals will come into action.

We will next examine the influence of the sun on continuous bridges whose upper or lower chords are covered by roadway planks or otherwise. In this climate, the power of the sun is great, as any one may feel on a hot summer afternoon by laying his hand on iron exposed to the direct rays of the sun. The difference in heat of iron thus exposed or shaded may be 30° or 40° Fahr. Suppose, therefore, the bottom chords of over 200 feet spans to be covered by planks, what would be the correction needed for a difference of temperature equal to 30° Fahr.?

Under this supposition, the girders, considered without weight, would rise so as to form part of a circle whose radius is $150\,000 \,.\, \frac{25}{30} = 125\,000$ feet. The rise in the centre of a chord of 400 feet would be, consequently, $\frac{200 \,.\, 200}{250\,000} = 0.16$ feet, or 1.92 inches. From Eq's *(VIII)* it is known, that this will cause a moment $M = 1\,350\,000 \,.\, 1.92 = 2\,592\,000$ pounds feet; this has a tendency to reduce the moment over the middle pier, which, for the unloaded bridge, was found equal to 2 966 000 pounds feet, and therefore, a small moment would remain, equal to 374 000 pounds feet, leaving still a pressure in the bottom chords over the middle pier of 393 pounds per square inch of section. The moment M, 2 592 000 pounds feet, would cause additional pressure on the end piers of 12 960 pounds,

and additional strains in end diagonals equal to about 10 per cent. of their maximum values.

If one span were fully loaded the maximum moment of 5 952 000 would be increased by $13\,000 \cdot \frac{5}{12} \cdot 200 = 1\,083\,000$ pounds feet, which is 18 per cent.

At the points where the moments change from positive to negative comparatively very great moments would be produced. Thus, at the third panel-joint from the middle pier, the greatest positive moment is 2 200 000 pounds feet, which would be increased by $13\,000 \cdot \frac{3}{4} \cdot 200 = 1\,950\,000$ pounds feet, so that the greatest positive moment at that point would be 4 150 000 pounds feet. In case the top chord were covered by floor planks, the moment M, 2 592 000 pounds feet, would cause tension in the top and compression in the bottom chords over the middle pier. The maximum moment, 8 430 000 pounds feet, would be increased more than 30 per cent. ; so that each degree Fahr. would cause one per cent. of additional strain. The negative moment at the second panel-joint from the middle pier would be increased from 3 230 000 to 5 400 000 pounds feet, that is, by 67 per cent.

If the temperature in the top chords raised to 40° Fahr., the central bearing could no longer act under the dead load only, for the truss would be $\frac{3}{8}$ inch above the bed-plate.

Arch bridges without hinges must be designed under the theory of continuity, with its defects and unfounded suppositions. Some of the objections against this theory as here applied have peculiar force—as the difficulty of proper manufacture, of close fit to the piers, and especially the influence of temperature. A few exclusively theoretical writers—on the authority of Oudry's experiments, and of thermometric measurements at the Tarascon bridge in France—deny the influence of heat on iron arch bridges with flat bearings. But they ignore the experience gained with the Theis bridge in Hungary, which might set at rest experiments with the thermometer.* This bridge changes its bearings on the abutments daily, and it is observed that the pressure moves from the lower

* Mr. Stoney thus remarks about the effect of temperature:—The rise in the crown of one of the cast iron arches of Southwark bridge for a change of temperature of 50° Fahr. was observed by Mr. Rennie to be about 1.25 inches ; the length of the chord of the estrados is 246 feet, and its versed sine, 23 feet 1 inch, and accordingly the length of the arch, which is segmental, is 3 020.8 inches. The range of temperature to which open work bridges, through which the air has free access, are subject in this country, seldom exceeds 81° Fahr. The range of temperature of cellular flanges, may, however, exceed that mentioned above, as Mr. Clark mentions that the temperature of the Britannia tubular bridge, before it was roofed over, differed " widely from that of the atmosphere in the interior, for the top during hot sunshine has been observed to reach 120° Fahr., and even considerably more ; and, on the other hand, a thermometer on the surface of the snow on the tube has registered as low as 16° Fahr."

chord bearing to the upper and back again. The bridge being by design very stiff, leaves its thrust bearings in winter, and unloaded, acts as a beam. It was anchored to the abutments the next summer after this observation was made, but during the following winter the piers commenced to move, whereupon the connections were removed. This example illustrates one of the practical difficulties inherent to continuous girders.

Most all European continuous bridges, as well as single span bridges, have either their bottom or their top chords protected from the sun's rays. Thus the Krementschug bridge (both railroad and roadway) has bottom chords covered by planking. The high iron viaducts in Switzerland and France (Freiburg, Busseau, Cére, &c.,) have superstructures of 7, 6 and 5 continuous spans, of which the top chords are protected by flooring. But we have seen that the strains of continuous bridges, whose bottom chords are overheated, will be greatly disturbed, and that for a difference in temperature of 30° Fahr. additional strains of 30 and even more than 50 per cent. may arise. I am not aware that this has been considered in the construction of continuous bridges.

Stone arches are affected in the same way as iron arches. With increased temperature the crown rises, and joints in the parapets over the crown open, while others over the springing close up. The reverse takes place in cold weather. In addition to the longitudinal movements to which all girders are subject from change of temperature, tubular girders move vertically or laterally whenever the top or one side becomes hotter than the rest of the tube. Referring to the Britannia tubular bridge Mr. Clark states that "even in the dullest and most rainy weather, when the sun is totally invisible, the tube rises slightly, showing that heat as well as light is radiated through the clouds. In very hot sunny days the lateral motion has been as much as 3 inches, and the rise and fall 2.3 inches. These vertical and lateral motions have not been much observed in lattice or open girder work, no doubt because the air and sunshine have free access to all parts (?) and thus produce an equable temperature."

James Hodges, in his work on the Victoria bridge, remarks: "In building the tubes the greatest increase of camber which occurred in one day consequent upon the difference of temperature between tops and bottoms of tubes was $1\frac{1}{4}$ inches; the barometer on the top reading 124°, in shade at bottom 90°, making a difference of 34°. The thermometer during the previous night was as low as 57°. It is therefore only fair to infer that as the bottom was in shade it would not be of the same temperature as the atmosphere, and that the increase of camber of $1\frac{1}{4}$ inches, was due to difference of temperature of probably as much as 50° Fahr."

The greatest longitudinal movement of roller beds was, for 258 feet $3\frac{1}{4}$ inches, due to a variation of from $-27°$ to $+128° = 155°$ Fahr. The greatest lateral movement caused by temperature was $1\frac{1}{4}$ inches.

The Victoria bridge is roofed over with timber and tin, and the temperatures measured probably were only those of the atmosphere, but not those of the iron. The girders being only imperfectly continuous, calculation of deflection of these girders is still less reliable than of theoretically designed work. Also draw-bridges (continuous over two spans) move sideways in consequence of one truss being more heated than the other. The side movement of a 360 feet draw was noticed as much as $1\frac{1}{4}$ inches and caused some trouble in locking. About equal vertical movements were noticed of a 360 feet draw, with planked floor. Mr. C. Shaler Smith (Transactions, vol. III, page 131, &c.) noticed alterations of the height of support caused by the sun and reversedly by cold winds—of as much as 1 inch. Mr. John Griffin informs me, that a 134 feet draw on the Philadelphia, Wilmington & Baltimore R.R. was so much affected by the unequal heating that it could not be turned, the deflection being about [illegible] inches. This he remedied by covering the top chord with wood.

We still have to compare the deflections of single and continuous spans. According to the theory, the deflection of a single span reaches its maximum under full load, which then is $\frac{5}{384} \frac{P\,l^3}{E\,I}$ in which P is the total load. For two continuous spans the maximum deflection occurs if only one span is loaded, and equals $\frac{4}{384} \frac{P\,l^3}{E\,I}$. These applied to the 200 feet spans give a deflection under live load for the continuous span of 2.08 and for the single span of 2.05 inches. The deflections are equal, so that also from this consideration there is no reason to prefer continuous girders. It would not be desirable to adopt these girders on this score even if the deflection were one-half less than for single spans, for we build single spans with a camber so as to make the floor just level under full proof load.

It is only with rafters and purlins of roofs made of shape iron of small depth that the consideration of continuity may lead to constructive advantages, and in some instances this consideration may be valuable in the construction of floors of bridges, but for bridge trusses of which we can vary the sections and can adopt a suitable depth, the question of reduction of deflection never arises, and continuity on this score need not be resorted to, either with girder or with arch bridges.

Finally, we find that the large continuous bridges over the Vistula, over the Rhine at Cologne, over the Dnieper at Krementschug, over the Danube at Pest, each of four spans, from 321 to 418 feet long, are only continuous over one support. *Theoretically*, there is perhaps a small advantage as to the average moment of flexure of girders stretched over more than two spans, though practically there is a loss of material at each change from concave into convex flexure. For two spans, there are two such regions, for three spans there are four, and for four spans there are six. Therefore, practically two spans are about as favorable in regard to the moment as three or four. Any how, the longitudinal expansion of the girders limits their length. For a maximum change of temperature equal to 150°, the change of length amounts to one-thousandth of the span, which for 400 feet spans amounts to 1.6 feet, so that at one end of such a bridge, provision must be made for a movement of 0.8 feet or 9.6 inches.

This consideration probably has limited the application of continuity to only two spans of great dimensions. There are, however, some bridges in Germany of seven and even nine (small) continuous spans, and in France and Switzerland are some large viaducts of five, six and seven

spans of 150 feet average length. Most all of these (viaduct of Fresburg, Busseau, Ceré, a bridge in Vienna, &c.) were built by iron works in France, and by Benkisser's method, the girders have been rolled over the piers, a method probably preferred on account of facility of erection, the works having a full plant for the purpose. On the other hand, most builders with whom the number of their orders is a consideration only second to quality and reputation, erect continuous girders on carefully built false works.

VII.—Recapitulation and Conclusions.

1°.—The mere theoretical calculation of the curves of moments and shearing forces of girders or arches without proper consideration of proportions, details and cost of manufacture, is exceedingly fallacious.

2°.—This fallacy will be the greater, if the theory by which the moments and shearing forces are calculated, stands upon false premises.

3°.—The theory of continuity is one of these theories, because it is based on the hypothesis of a constant modulus of elasticity, which, as proved, does not agree with the nature of the material. It has been shown that the modulus of wrought iron varies from 17 000 000 to over 40 000 000 pounds per square inch.*

4°.—Even if it were assumed that the modulus had a constant value, still a correct theory would require that there be but one system of diagonals in the web of a continuous girder. Under an arbitrary supposition, the strains in the diagonals and posts of continuous girders with two or more systems, cannot be calculated, but only guessed.

5°.—The theory neglects the influence on the moments and shearing forces caused by the deflections due to the extensions of the web ties, and to the compressions of the web struts.† The theory also needs a correction if the chords are varied.

6°.—The correct application of the principle of continuity involves an exceedingly tedious labor, and, if generally introduced, would greatly impede the business of bridge construction in this country.

7°.—In the determination of the sections of chords and webs, it must be considered that a member exposed to tension as well as to pressure, must be proportioned to resist the maximum tension plus the maximum pressure.

8°.—Continuous girders require very accurate workmanship, both in the shops and in the field, which, if exacted by the inspecting engineer,

* Page 161. † In Appendix B we propose to prove, mathematically, that this influence is considerable, and upsets the common theory.

will cause a greater expense than that for single spans. The connections at the points where the strains change from the positive to the negative must be made with more care than if tension or only compression had to be resisted. ·Especially, in case of riveting, the holes must in the field be rimmed to match perfectly, the rivets placed more closely and driven thoroughly.

9°.—The foundations and masonry of piers on which continuous girders shall be placed, must be of excellent quality. Single span trusses may, without injury, be placed on piers which have settled several inches; but this is not the case with continuous girders. Engineers contemplating the use of continuous girders should realize the necessity of this provision, and previously estimate the additional cost of substructure.

10°.—If it is intended to roll continuous girders on to the piers, ordering and inspecting engineers should examine carefully whether the contractor has calculated the extra strains arising from the weight of the projecting cantilever, has properly reinforced the posts and introduced additional diagonals and chord material at the points of change of flexure.

11°.—Continuous girders improperly built or placed on their bed-plates, have to resist greater strains than contemplated, which, for one inch difference in height of location of bed-plate, on the middle pier of a 200 feet span, is increased 16 per cent.

12°.—If the upper or the lower chords of a continuous bridge are protected from the direct heat of the sun, the strains are much disturbed and (for 30° difference of temperature) may over the middle pier be increased 30 per cent. and at the points of change of flexure more than 50 per cent., and the structure may even rise from the middle piers, notwithstanding its dead load.

13°.—The proportions of depth of span to height depend essentially on the system and on the details used. The lighter theoretically and practically the web can be made, the greater the height can be chosen, which is only limited by the practicable length of web members and by the calculation of the strains, sections and weights due to the effect of wind. Practically, the best depth is obtained if an additional foot increases the weight of the total structure. Continuous girders, requiring more material in their webs than single spans, cannot be built as high. European bridges having been built too shallow for single spans, as far as economy is concerned were better proportioned when built continuously. This is one of the reasons why, in Europe, continuous bridges proved to be the lighter.

Properly proportioned single spans on the same system, at least, should be no heavier.

14°.—We have found by investigating the example of two 200 feet spans, that properly designed single spans with American details, are actually lighter than continuous girders. The bridge of Buda-Pest and the Krementschug bridge are examples of continuous structures of economical European construction, but they do not compare either in cheapness or quality with single spans, well proportioned, having the most scientific American details.

15°.—Continuous bridges deflect as much as single spans of correspondingly greater depths. It has now been proved that the theory of continuity, most interesting as it is in a scientific point of view, nevertheless forms only a part of pseudo-science ; being based on false suppositions, it is too delicate in execution and under use, and finally, because it is not economical. Practical constructions are designed with a certain factor of safety. The truer the theory, the easier its suppositions can be fulfilled, the less its results are modified by disturbing influences; the less it is influenced by the unreliable incidents of the application of human labor, the more reliable a construction will be, and the smaller the factor of safety may be taken.

It has been shown, that by theory we cannot gain greater perfection in practice, unless we constantly are comparing the results of our deductive investigations with experimental facts.* Results of such experiments on executed continuous girders are not known. All that we have, are some notes on deflections of finished bridges under test loads. We usually learn that the deflections were much less than expected or calculated, which is communicated as a proof of the excellency of workmanship, as if workmanship could reduce the extensions or compressions of iron—in other words, could raise the modulus. In this country, continuous girders have only been used for drawbridges. The calculations of the strains of these continuous girders are still considerably more complicated, more delusive, and more untrustworthy than those made for fixed bridges.

It is not intended to enter into these mathematics. It only is mentioned that, probably, Prof. Sternberg was the first engineer who applied the theory of continuity to the various suppositions as regards dead load of fixed and swinging draw, of partial and full live load, the draw being screwed up at ends or loose, &c. The pivot bridge, by him was consid-

* Compare what Mr. B. Baker says, pages 221, 228 and 313, "Strength of Beams, Columns and Arches. London. 1870."

ered as a continuous girder over three openings, two large outer spans and a short middle part. Herr Sternberg applied the formula thus obtained to the draw of Kustrin, in Prussia.* The central part of the draw really constituting a separate part of a lattice beam composed of chords and lattice diagonals, this calculation was justified. With large draws, as built in this country, the calculation based on three openings can be reduced to that for two spans so long as the two centre diagonals, only necessary to give stiffness during the movement of the swing bridge, are provided with elastic sling loops, or any other suitable elastic medium. The half loaded draw will depress somewhat the drum, the wheels and even the masonry, and the light diagonals being incapable of taking up any great amount of strain without stretching, the other bearing on the round pier, will remain in action. The two chords above the round pier will sensibly experience the same amount of stress. It is even admissible to slacken these diagonals when the draw is fixed, so that the bridge may act by a scientifically correct general arrangement of modified continuity, and before the bridge is to be turned, by some arrangement, the diagonals might be brought into action. By such construction the greater part of the weight would not be thrown on only a few wheels. The great and wholly unnecessary complicity of the calculation of a bridge resting on four supports, can be dispensed with, so much the more since the whole theory, under all circumstances, deserves but very little confidence.

The investigation which is now finished will surely not impair confidence in the construction of bridges whose design is exclusively based on the plain, unmistakable law of the lever, which can be calculated in a short time, be easily manufactured and erected.

If engineers wish to build continuous girders, they will do better to use continuous bridges with hinges, when they will escape all uncertainties caused by defects of theory, by inequality of moduli, by several systems of diagonals, by inequality of heights of supports, of additional strains caused by heat of sun, &c. Practical men will welcome such simplicity, notwithstanding it may not satisfy a few mathematicians, because the problems connected therewith, to them, may not seem sufficiently interesting.

* His whole investigation was published in the report of the Kreutz Kustrin R. R. of 1857.

Appendix A.

Torsional Experiments with Phœnix Iron from Ruhrort.

The bars were twisted forward and backward, each particle being alternately strained by tension and by compression; (all strains are in pounds per square inch).

Strains.			Repetitions which produced Rupture.	Strains.			Repetitions which produced Rupture.
From Tension	to Compression.	Sum.		From Tension	to Compression.	Sum.	
— 35 200	+ 35 200	70 400	56 430	— 24 200	+ 24 200	48 400	3 632 588
— 33 000	+ 33 000	66 000	99 000	— 22 000	+ 22 000	44 000	4 917 992
— 28 600	+ 28 600	57 200	479 490	— 19 800	+ 19 800	39 600	19 186 791
— 26 400	+ 26 400	52 800	909 810	— 17 600	+ 17 600	35 200	132 250 000 not broken.

Experiments on Flexure with same Iron.

Each particle had to stand but one kind of strain, either tension or compression.

Strain.	Repetitions which produced Rupture.	Strain.	Repetitions which produced Rupture.
60 500	169 750	39 600	4 035 400
55 000	420 000	35 200	3 420 000
49 500	481 975	33 000	4 820 000 not broken.
44 000	1 320 000		

Flexure of Homogeneous Iron.

Melted wrought iron, from Pearson, Coleman & Co., in Hull. Variable flexures were added to permanent ones.

Permanent Strain.	Variable Strain.	Maximum of both.	Repetitions which produced Rupture.
44 000	44 000	88 000	475 500
33 000	44 000	77 000	1 234 600
	44 000	44 000	34 500 000 not broken.

Phœnix Iron under repeated Extensions.

Tensile Strains.		Repetitions until Rupture took place.	Tensile Strains.		Repetitions until Rupture took place.
From permanent Strains	to		From permanent Strains	to	
0	52 800	800	0	39 600	480 852
0	48 400	106 910	0	35 200	10 141 645
0	44 000	340 853	22 000	48 400	2 373 424
0	39 600	409 481	26 400	48 400	4 000 000 not broken.

TORSIONAL EXPERIMENTS ON STEEL.
Each particle under tension and compression.

From Tension	to Compression	Sum.	Repetitions to produce Rupture.	
— 35 200	+ 35 200	70 400	642 675	Krupp's Steel.
— 35 200	+ 35 200	70 400	3 114 160	Krupp's Steel.
— 33 000	+ 33 000	66 000	4 163 375	Krupp's Steel.
— 33 000	+ 33 000	66 000	45 050 640	Krupp's Steel.
— 24 200	+ 24 200	48 400	19 100 000	not broken. Bochum Steel.
— 35 200	+ 35 200	70 400	627 000	Bochum Steel.
— 35 200	+ 35 200	70 400	20 467 780	Bochum Steel.
— 33 000	+ 33 000	66 000	2 845 250	Bochum Steel.
— 33 000	+ 33 000	66 000	57 360 000	not broken. Bochum Steel.
— 28 600	+ 28 600	57 200	14 176 171	not broken. Bochum Steel.
— 33 000	+ 33 000	66 000	1 023 625	Borsig steel.
— 26 400	+ 26 400	52 800	3 275 860	Vicker's steel.
— 24 200	+ 24 200	48 400	8 660 000	Vicker's steel. not broken.
— 26 400	+ 26 400	52 800	859 700	broken (Krupp's).
SAME EXPERIMENTS ON HOMOGENEOUS IRON; (melted wrought iron).				
— 28 600	+ 28 600	57 200	636 500	
— 26 400	+ 26 400	52 800	3 930 150	

EXPERIMENTS ON FLEXURE WITH STEEL.—Krupp's Railroad Axles.

DIRECT STRAINS.		Repetitions producing Rupture.	DIRECT STRAINS.		Repetitions producing Rupture.
From	To		From	To	
0	60 500	1 762 300	0	55 000	5 234 200
0	57 750	1 031 200	0	55 000	40 600 000 not broken.
0	57 200	1 477 400	....	....	....
Bochum's Railroad Axles.					
0	77 000	104 300	0	55 000	729 400
0	66 000	317 275	0	55 000	1 499 600
0	60 500	612 500	0	49 500	43 000 000 not broken.
Krupp's Spring Steel,					
0	110 000	39 950	0	55 000	40 600 000 not broken.
0	88 000	117 000	0	49 500	32 942 000 not broken.
0	66 000	468 200	....	....	(Experiment was ended)
88 000	132 000	35 600 000 not broken.	44 000	88 000	38 000 000 not broken.
99 000	132 000	33 478 700 broken.	61 600	88 000	36 000 000 not broken.
72 600	110 000	19 673 300 not broken.	27 500	77 000	36 600 000 not broken.
66 000	99 000	33 600 000 not broken.	33 000	77 000	31 150 000 not broken.
44 000	88 000	35 800 000 not broken.	....	....	....

APPENDIX B.

In the foregoing pages, acquaintance with the theory of the elastic line of the beam was supposed. It will now be shown how this curve may be determined in a skeleton structure, by means of a simple trigonometrical consideration.

Fig. 17.

Let $A\ B\ C$ be a triangle whose sides $a\ b\ c$ have been altered by very small quantities Δa, Δb, Δc; the problem is to find the alteration Δx of an angle. We have $a^2 = b^2 + c^2 - 2\,b\,c\,.\cos.\,x$, which, by inserting the differences, leads to $(a + \Delta a)^2 = (b + \Delta b)^2 + (c + \Delta c)^2 - 2\,(b + \Delta b)\,(c + \Delta c)\cos.\,(x + \Delta x)$.

By developing this equation and considering that the squares of differences are very small quantities in comparison with their first powers, we get:

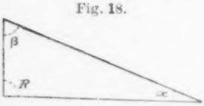
$$a\,\Delta a = b\,\Delta b + c\,\Delta c - (b\,\Delta c + c\,\Delta b)\cos.\,x + b\,c\,\sin.\,x\ \Delta x\,.$$

Hence we derive the value of Δx.

$$\Delta x = \frac{a\,\Delta a - b\,\Delta b - c\,\Delta c + (b\,\Delta c + c\,\Delta b)\cos.\,x}{b\,c\,.\sin.\,x}$$

By applying this formula to a rectangular triangle the formula is simplified into:

Fig. 18.



$$\left.\begin{aligned} \Delta x &= \frac{\Delta a}{b} - \frac{\Delta d}{d}\,.\,\frac{a}{b} \\ \Delta \beta &= \frac{\Delta b}{a} - \frac{\Delta d}{d}\,.\,\frac{b}{a} \\ \Delta R &= -\frac{\Delta a}{b} - \frac{\Delta b}{a} + \frac{\Delta d}{a}\,.\,\frac{d}{b} \end{aligned}\right\} \quad (1.)$$

The sum of $\Delta x + \Delta \beta + \Delta R = 0$, as expected.

These few formulæ (1) are sufficient to calculate the angles of deflection at a joint of a properly built skeleton bridge.

Fig. 19.

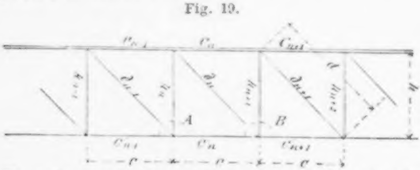


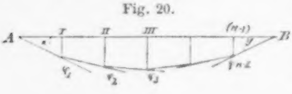
Fig. 19 represents part of a quadrangular truss, whose panels have the length c, whose height is h, and whose diagonals are of the length d. The truss being under transverse strain, receives alterations of the lengths of its members and consequently of its angles. The angle A originally was 180 degrees, we now have to calculate the small alteration γ_n of this angle. This alteration is the sum of the alterations of the three angles around A, namely:

$$\gamma_n = \frac{\Delta c_{n-1} - \Delta c^1{}_n}{h} + \frac{\Delta h_{n-1} - \Delta h_n}{c} - \left(\frac{\Delta d_{n-1} - \Delta d_n}{h\,c}\right).\,d \qquad (2.)$$

In this expression $\dfrac{\Delta c_{n-1} - \Delta c^1{}_n}{h}$ representing the influence of the chords c and c^1, is a sum, because if c_{n-1} is under compression $c^1{}_n$ will be under tension and the absolute values of the alterations of these quantities will add together. The influence of the posts $\dfrac{\Delta h_{n-1} - \Delta h_n}{c}$ and of the diagonals $(\Delta d_{n-1} - \Delta d_n)\,\dfrac{d}{h\,.\,c}$ are actual differences of absolute numbers. Hence it follows that, generally speaking, the influence of the chords on the value of deflection must be more important than the influence of the web members.

The theory of the elastic line, such as developed with the integral calculus, throws off the influence on γ caused by the posts and diagonals, whilst only the chords are considered. It is our object to now show that this neglect leads to appreciable errors. Suppose the originally straight bottom chord of a beam has deflected and the angles of 180° at I, II, III. . . . have altered by the values $\gamma_1, \gamma_2, \gamma_3, \ldots \gamma_{n-1}$, which alterations here are negative values.

Fig. 20.



The question arises which will be the angles x and y. The originally horizontal chord pieces $c_1\ c_2\ c_3\ \ldots$ will form angles with the line $A\ B$, as follows:

x, $(x + \gamma_1)$, $(x + \gamma_1 + \gamma_2)$, $(x + \gamma_1 + \gamma_2 + \gamma_3)$, . . . $x + \gamma_1 + \gamma_2 + \gamma_3 \ldots + \gamma_{n-1})$.

Here also exists the equation:

$$\left.\begin{aligned} -(x+y) &= (\gamma_1+\gamma_2+\gamma_3 \;\ldots\; +\gamma_{n-1}) \\ \text{so that } -y &= x+\gamma_1+\gamma_2+\gamma_3 \ldots\ldots +\gamma_{n-1}. \end{aligned}\right\} \quad (3.)$$

The chord pieces $c_1\, c_2 \ldots$ being equally long, the sum of the sines of the angles which are formed by $c_1\, c_2 \ldots c_{n-1}$, with the horizontal line $A\,B$ must be equal to zero. And since the angles are very small, their sines can be put equal to the angles themselves. Consequently we arrive at this equation:

$$0 = x + (x+\gamma_1) + (x+\gamma_1+\gamma_2) + (x+\gamma_1+\gamma_2+\gamma_3) + \ldots + (x+\gamma_1+\gamma_2+\ldots + \gamma_{n-1}) \quad (4.)$$

or,

$$-n\,x = (n-1)\,\gamma_1 + (n-2)\,\gamma_2 + (n-3)\,\gamma_3 + \ldots + 2\,\gamma_{n-2} + \gamma_{n-1}.$$

and likewise we have: $-n\,y = (n-1)\,\gamma_{n-1} + (n-2)\,\gamma_{n-2} + (n-3)\,\gamma_{n-3} + \gamma_2 + \gamma_1.$

In case of the span $A\,B$ being uniformly loaded and supported on both ends the angles x and y would be equal, and since the sum $x+y$ equals the negative sum of the angles $\gamma_1+\gamma_2+\ldots\gamma_{n-1}$ each one would be half this sum. By inserting the values of equation (2) in the expression for $x+y$ of a uniformly loaded truss, all Δh_n and Δd_n will disappear, with exception of the influence of the end posts and of the end diagonals of each system. The chords, however, will remain in the formula for x and y under any circumstances. Of a uniformly loaded beam, resting upon two supports, the influence of the web on the angles x and y is as follows:

$$-x = -y \text{ due to web} = \tfrac{1}{2}\left[\frac{\Delta h_o}{c} + \frac{\Delta h_n}{c} - (\Delta d_o + \Delta d_n)\,\frac{d}{h\,c}\right]$$

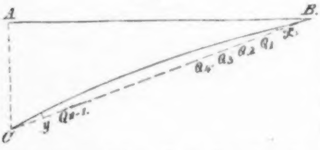
in which expression Δh_o and Δh_n are negative values.

The total expression is negative, so that the influence of the web increases the angles x and y.

The web has a very considerable influence on the angle of deflection of a cantilever beam. The originally straight beam $A\,B$ being fixed at B is bent into the curve B 1, 2, 3, 4, C, when the angle x will be equal to

$$-\left[\frac{1}{n}\Big((n-1)\,\gamma_1 + (n-2)\,\gamma_2 + \ldots 2\,\gamma_{n-2} + \gamma_{n-1}\Big) + \text{angle } I\,B\,A\right]^*$$

Fig. 21.



In this sum no post and no diagonal of the truss AB will disappear.

If we now suppose the special cause of interest that the cantilever AB in A respectively, C is acted upon by a single weight P, we know from the law of the lever that the chords are strained the more, the further they are from A. Hence we have to multiply P with its lever arm, from A to the chord piece in consideration, to divide by the section of the member and by the modulus. Hence the value of compression or tension of a chord piece C_n equals $\dfrac{c.\,P.\,n.\,c.}{Section \times E.\,h}$ so that the factor $\dfrac{c^2\,P}{E \times section}$ is common to all extensions and compressions provided the sections are taken as constant, which, as well known, is one of the principal hypotheses of the ordinary theory of continuity. By inserting this expression into formula (4) w being the section, we get:

$$x = -\frac{2\,c^2\,P}{n\,E.\,w.\,h^2}\cdot\Big[(n-1)^2 + (n-2)^2 + \ldots 9 + 4 + 1\Big]$$

$$x = -\frac{2\,c^2\,P}{n.\,E\,w.\,h^2}\cdot\frac{(n-1)\,.\,n\,.\,(2n-1)}{1\,.\,2\,.\,3}.$$

If n becomes a very large number $\dfrac{(n-1)\,.\,(n)\,(2n-1)}{6}$ turns into $\dfrac{n^3}{3}$ and the angle $x = \dfrac{P\,.\,(c\,n)^2}{3\,.\,E\,.\,\dfrac{w\,h^2}{2}} = \dfrac{P.\,l^2}{3\,.\,E\,.\,I}$ where $\dfrac{w\,h^2}{2}$ = the moment of inertia I, and $cn = l$ = the length of the span. This formula was one on which we based our method of the treatment of the common theory of continuous girders. Its use involves the supposition that the extensions of the diagonals, and the compressions of the posts, are immaterial in regard to the angles of deflection.

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* Angle $I\,B\,A$ is the angle which the curve makes with $A\,B$ at B.

Since we know that the angles $x + y$ equal the sum of all angles γ, y can readily be derived from x, and there will be $y = \frac{2\,c^2\,P}{E\,.\,w\,.\,h^2}\,.\,\frac{n^2}{2} - \frac{2\,c^2\,P}{E\,.\,w\,.\,h^2}\,.\,\frac{n^2}{3} = \frac{P\,.\,l^2}{6\,.\,E\,.\,I}$. This was the other of two Eq's (*III*).

In order to prove by figures the influence of the web system on the angles of deflection, the example of the two 200 feet spans is chosen. The chords of this design are almost of equal section. This average section applied to the general formula for the angles γ and δ under supposition of the full load, 1 200 pounds, for dead load and 2 240 pounds for the movable load per foot. There results $\gamma = \delta = \frac{3\,440\,.\,200\,.\,200\,.\,260\,.\,2}{2\,.\,24\,.\,30\,000\,000\,.\,25\,.\,25\,.\,25} = \frac{5.87}{2\,400}$ where the divisor 2 400 is the length of the span in inches. The angle of deflection $\frac{5.87}{2\,400}$ must be increased by the influence of the posts and diagonals. This correction amounts to $\frac{3.79}{2\,400}$, or about 61 per cent. of the angle due to the chords alone.

The angle of elevation caused by the chords under action of the total force $p = 42\,630$ equals

$$\frac{42\,630}{3}\,.\,\frac{200^2}{30\,000\,000\,\,.\,\,25\,\,.\,\,\frac{25\,\,.\,\,25}{2}} = \frac{5\,.\,82}{2\,400}$$

This value is so nearly equal to $\frac{5{,}87}{2\,400}$ found to be the angle of deflection due to the chords alone as caused by the full load on the two spans, that—were it not for the web system—we should feel very much satisfied with the exactness of the theory.

But the force $42\,630 = p$ also causes extensions and compressions of the web members which result in the angle of elevation $\frac{1.7}{2\,400}$, amounting to 30 per cent. of the angle caused by the chords.

We have now the angles

	Caused by chord.	Caused by web.	Total.
of deflection	$\frac{5.87}{2\,400}$	$\frac{3.79}{2\,400}$	$\frac{9.66}{2\,400}$
of elevation	$\frac{5.82}{2\,400}$	$\frac{1.70}{2\,400}$	$\frac{7.52}{2\,400}$

The total angles should be equal, but they differ in reality by 29 per cent. The angle of elevation is too small; in other words, the force 42 630 is found too small, or consequently the moment over the middle pier is calculated too small. The webs of the calculated 200 feet spans are too strong at the end piers, and they are too weak at the central pier. The chords in the middle of the spans such as designed are too large. The point of contrary flexure is nearer to the centre of each span than anticipated.

In short, the application of the common theory of continuity to skeleton structures is not justified. Whilst continuity of rolled beams or of plain plate girders of uniform section and small depth in some constructions may be used with advantage, the theory of continuity should not be applied to deep trusses with variable sections of chords, posts and diagonals, and of surely different moduli; so much the less, since true economy refers us to single spans, designed exclusively on the principle of the lever.

The most economical structures fortunately require but very little calculation, so that estimates can be made within a few hours, without formulæ or drawings. All that is necessary is a piece of paper and a pencil in the hand of a thinking bridge engineer, who in the school of practice has learned to sift rubbish, both analytical and graphical, from the few principles of natural philosophy which are really needed, which are commercially applicable and from which, by plain reasoning, special rules readily can be derived whenever desirable.

CXXIII.

NOTE ON

THE RESISTANCE OF MATERIALS,

AS AFFECTED BY FLOW AND BY RAPIDITY OF DISTORTION.

A Paper by Prof. ROBERT H. THURSTON, Member of the Society.

PRESENTED MARCH 1st, 1876.

The effect of the "Flow of Metals" and of the force of polarity described by Prof. Henry, in modifying their resistance to external stress and their strain, was alluded to by the writer in preceding Transactions, as follows :*

"The same molecular movement, or flow, which rearranges the internal force and relieves internal strain, may be a phase of that viscosity which Vicat supposed might in time permit rupture of metal subjected to stress nearly approaching its original ultimate resistance, the one action being a more immediate result than the other, and the latter producing its effect, even when cohesive force may have been actually intensified."

It was noted, however, that, in all cases in which wrought iron and steel had been subjected to stress exceeding the elastic limit, the metal had exhibited no tendency to flow, and that, in nearly every case observed, an actual "elevation of the elastic limit by strain" had taken place. No experiment had then been made by the writer in which the same sample had exhibited both the elevation of the elastic limit by strain and the phenomenon of flow.

Since that time, when experimenting upon copper, strain-diagrams produced automatically have been observed to exhibit this double effect. The elevation of the elastic limit has occurred in the earlier part of the test, and, at a later period, the strain-diagram exhibits flow, the metal yielding under a gradually decreasing stress. The progressive distortion which had never been observed by the writer in iron or steel, has, since the date of the paper, been frequently noted in other materials. For example, the following are a few illustrations †:

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* LXXXII. On the mechanical Properties of Materials of Construction. Vol. III, page 13.

† Selected from the record books of the Mechanical Laboratory of the Stevens Institute of Technology.

Tests by Torsion.

Number of Test.	Material. Parts Tin.	Material. Parts Copper.	Time under Stress.	Angle of Torsion.	Fall of pencil.	Remarks.
1 } Same piece.	....	....	40 hours.	65°	0.06 inches.	Recov'd after further distortion of 1°.
2 } Same piece.	100	....	1 hour.	180°	0.1 "	" in 8°.
3 } Same piece.	......	....	2 hours.	280°	0.1 "	" in 80°.
4	99.44	0.56	12 min's.	* 380°	50 per cent.	Did not recover.
5	98.89	1.11	....	....	....	Behaved like No. 4.
6	Alloy.		....	58°	0.2 inches.	Did not recover.

Tests by Transverse Stress,—with Dead Loads.

Samples 1 × 1 × 22 inches.

Number of Test.	Material. Parts Tin.	Material. Parts Copper.	Load. Pounds.	Deflection. Inches.	Time	Increased Deflection. Inches.	Breaking Weight. Pounds.
7	....	100	600	0.534	5 minutes.	0.009	650
8	1.9	98.1	475	1.762	3 "	0.291	......
			500	2.108	3 "	0.488	500
9	7.2	92.8	950	0.348	5 "	0.081	1 350
10	10.	90.	950	0.395	5 "	0.021	......
			1 485	3.447	13 "	4.087	1 485
11	90.3	9.7	100	0.085	10 "	0.021	.....
			120	0.14	10 "	0.055	......
			140	0.221	10 "	0.098	......
			"	0.319	10 "	0.038	......
			"	0.357	40 hours.	0.92	......
			160	1.294	10 minutes.	0.025	......
			"	1.32	1 day.	1.	......
			"	2.32	1 "	1.	......
			"	2.32	1 "	1.	160
12	98.89	1.11	90	0.243	5 minutes.	0.063	......
			120	0.736	15 "	1.055	......
			"	1.791	30 "	0.748	......
			"	2.539	45 "	0.595	......
			"	3.134	12 hours.	8.	120
13	100		80	0.218	5 minutes.	0.064	110

* Taking elasticity line.

Metals having a composition intermediate between these extremes have not been observed to exhibit flow or to increase deflection under a constant load.

Tests by tension with similar materials exhibit similar results, and these observations and experiments thus seem to confirm the remarks of the writer as above quoted, and to indicate that, under some conditions, the phenomena of flow and of elevation of the elastic limit by strain may be co-existent and that progressive distortion may occur with "viscous" metals.

The paper referred to, enunciated a principle which had been deduced from experiments on wrought iron which is, if possible, of more vital importance to the engineer than the facts just given, viz.: "That the time during which applied stress acts, is an important element in determining its effect, not only as an element which modifies the effect of the *vis viva* of the attacking mass and the action of the inertia of the piece attacked, but, also, as modifying seriously the conditions of production and relief of internal strain by even simple stresses."*

It was then shown, by autographic strain-diagrams, that some materials yield the more readily the more rapidly the distortion and rupture are produced, their resistance varying in some inverse ratio with the rapidity of change of form. It was further suggested that this action might be closely related to the opposite phenomenon of the elevation of the elastic limit by strain. An explanation was offered in the theory that, with rapid distortion, insufficient time is allowed for the relief of internal strain in materials capable of exhibiting that condition. It was further remarked that "the most ductile substances may exhibit similar behavior, when fractured by shock or by any suddenly applied force, to substances which are comparatively brittle," and illustrations were given of such behavior, and the precautions to be taken by the engineer, in view of this important modification of the resistance of materials by velocity of rupture, were stated.

The writer has continued his experimental researches, with occasional interruption, since that time, and has found the above given statements confirmed, and that relations exist between these phenomena of strain and the time under stress, which may properly be stated here as complementary of the principles already published in the two preceding notes which have appeared in Transactions.†

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* Vol. III, page 30. † LXI. Vol. II, page 239. CXV. Vol. IV, page 334.

Should it be true, as suggested by the writer, that the cause of the decreased resistance, sometimes observed with increased velocity of distortion, is closely related to the cause of the elevation of the elastic limit by strain,* it would seem a simple corollary, that *materials so inelastic and so viscous as to be incapable of becoming internally strained during distortion should offer greater resistance to rapid than to slowly produced distortion*, in consequence of their inability to "flow" so rapidly as to reduce resistance by such fluxion at the higher speed, or by correspondingly reducing the fractured section. This principle has been shown, by a large number of experiments, to be frequently, if not invariably, the fact. Copper, tin and other inelastic and ductile metals and alloys are found to exhibit this behavior, and are, therefore, quite opposite in this respect to ordinary wrought iron and worked steel.

The writer has noted the fact that very soft wrought iron does not always exhibit an observable elevation of the elastic limit by strain, and Com. L. A. Beardslee, U. S. N.,† has recently observed that the softest and most ductile specimen of iron yet tested by him at the Washington Navy Yard exhibited a perceptible increase of resistance with a considerable increase of rapidity of extension. This metal was peculiar in its softness and extreme extensibility. All the irons of commerce appear to belong to the other class.

The records of the Mechanical Laboratory of the Stevens Institute of Technology frequently illustrate the proposition that metals which gradually yield under a constant load offer increased resistance with increased rapidity of rupture.

The curves of deflections of a considerable number of ductile metals and alloys are very smooth when the time during which each load has been left upon them is the same; but, whenever that time has been variable, the curve has been irregular. Bars of such metals broken by transverse stress give a greater resistance to rapidly increasing stress than to stress slowly intensified. Two pieces of tin from the same bar were broken by tension, the one rapidly and the other slowly. The first broke under a load of 2 100 and the latter of 1 400 pounds. The example illustrates well the very great difference which is possible in such cases, and seems, to the writer, to indicate the possibility in extreme cases of obtaining results which may be fatally deceptive when the time of rupture is not noted.

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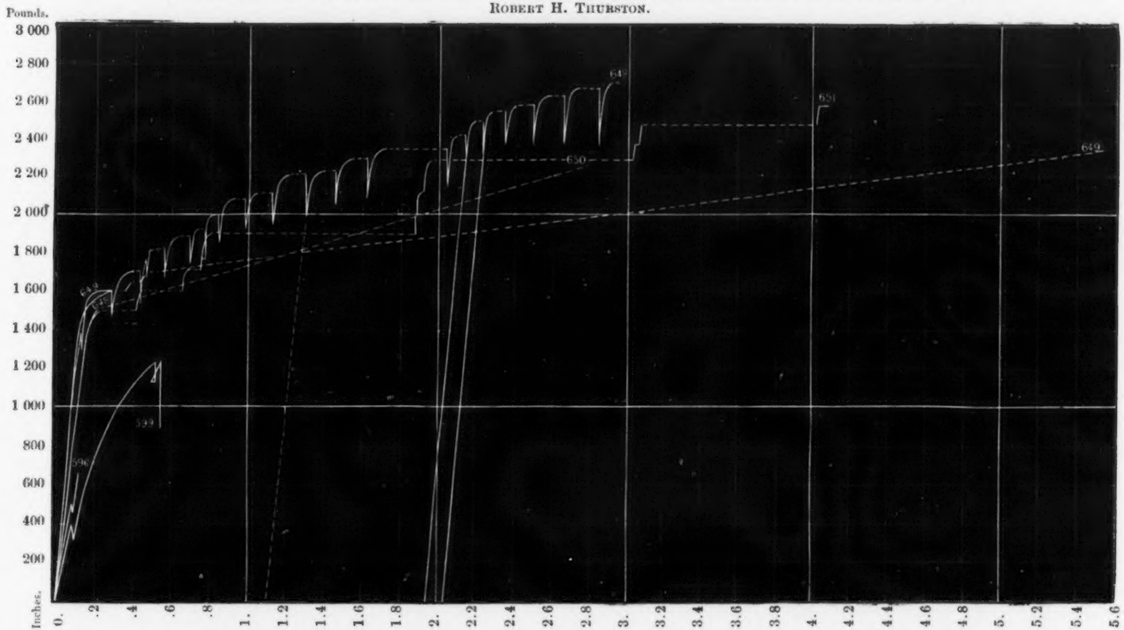
* Transactions, Vol. III, page 363. † Whose work has been referred to, in earlier papers.

STRAIN DIAGRAMS

OF FOUR BARS OF WROUGHT IRON, AND TWO OF COPPER-ZINC ALLOYS TESTED BY TRANSVERSE STRAINS.

ROBERT H. THURSTON.



Autographic strain-diagrams, given by this class of metals, exhibit smooth, straight and horizontal lines for long distances on the paper where the distortion is produced by a uniform motion. Increasing the rapidity of distortion causes an immediate and sustained elevation of the pencil, and a decrease of velocity causes the line to droop to a lower level. In some experiments* a torsion of one revolution in a half hour, the test piece being $\frac{5}{8}$ inch diameter and one inch long, just kept the pencil on a horizontal line.

Two test pieces from the same bar were broken, the one rapidly, the other slowly. The former gave a strain-diagram of which the maximum ordinate was about 50 foot pounds higher than the maximum of the latter, the difference being nearly 50 per cent. of the higher.†

It is evident that, whatever the character of the material and whatever the velocity of rupture, the effect of the *inertia* of the mass, and of particles not immediately affected by a shock, remains, and that its effect is *always* to reduce the resilience of the metal and its resistance to shock; and this reduction may, in many cases, more than compensate the increase of resistance here noted. Its tendency is always to produce a sharp fracture which, with such sudden blows as are given by cannon shot, for example, may resemble the break characteristic of brittle and non-ductile substances.

The writer would, therefore, divide the metals used in construction into two classes:

1st. Metals subject to internal strain by artificial manipulation and which may exhibit an elevation of the elastic limit by strain and decreased power of resisting stress under increasing rapidity of distortion. The ordinary irons of commerce are typical of this class.

2d. Metals of an inelastic viscous character, not subject to internal strain and not usually exhibiting an elevation of the elastic limit by strain and which offer increased resistance when the rapidity of distortion is increased. Tin is a typical example of this class.

It is obvious that the value of the former class for the construction of the engineer is vastly greater than the latter, and especially for permanent loads and low factors of safety.

---

* Made for the writer with great care and skill by his assistant, Mr. Wm. Kent.

† The inertia of the *weight* in these examples, has no measurable effect in modifying those results.

The depression of the elastic limit has been observed previously in materials, but less attention has been paid to it than the importance of the phenomenon would seem to demand. The accompanying plate exhibits the strain diagrams produced by plotting the results of experiments.* They are selected as typical examples, and as representing the two classes of materials described.

In making the experiments the bar was mounted on cylindrical steel bearings, which were themselves supported on accurately planed level surfaces, and the deflection was produced by means of a powerful screw and a large hand-wheel. The weight was measured by a Fairbanks scale combination, and the deflections and sets by a special measuring apparatus† which reads to 0.0001 inch, with an error of 0.000741. Touch is indicated by a delicate Stackpole level. The measuring instrument was unaffected by the forces tending to distort the straining apparatus. The deflecting force was adjusted by the scale-beam. The bar being in place, the weight to be put on it was set off on the scale-beam, and the screw was carefully turned until, by its pressure on the middle of the bar, the scale-beam slowly rose and vibrated about the middle of its range, which point was indicated by a pointer at the end of the beam, traversing a fine lined scale on the frame. When the adjustment had become satisfactory, the deflection was read off and the beam usually released, in order that the set might be observed. It was then again deflected by a heavier weight. Occasionally the bar was left thus strained, and with a constant deflection, for a considerable period of time, and the change of effort exerted by it noted at frequent intervals. In all such cases the scale-beam gradually drooped, and a decreased effort to effect restoration of form was indicated. When the beam had fallen, the weight was pushed back until the beam arose and vibrated about the centre line again, and the weight and time were recorded. This was repeated as the beam exhibited less and less loss of power of restoration, and when this decrease of effort no longer exhibited itself, a new series of deflections was produced.

The bar No. 599, which was quite ductile, exhibited an unchanged law of relation of amount of deflection to intensity of deflecting force, and, as shown by the diagram, the curve representing its test pursued the same general direction after one of these "time-tests" as before.

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* Made and recorded in the Mechanical Laboratory of the Stevens Institute of Technology.
† Made to the order of the writer, by Mess. Brown & Sharpe.

The loss of effort at 163 pounds is seen to have been about 20 pounds, the deflection amounting to 0.0347 inches, and the effort falling from 163 to 143 pounds. At 403 pounds the loss of restorative force is about the same; the figures fall from 403 to 333 pounds, the deflection being held constant at 0.0886 inches, again from 333 to 302 pounds at a deflection of 0.0896, and still again from 1233 to 1137 pounds at a deflection of 0.5209 inches.

Before the bar, under further deflection, had quite regained its original resisting power, the "time-test" was repeated, the deflection amounting to 0.5456 inch, and the weight applied being 1233 pounds. The result noted was quite unanticipated. The effort steadily decreased at a varying rate, which is indicated by the diagram of time and loads, and the bar finally snapped sharply, and the two halves fell upon the floor. The effort had decreased to 911 pounds. The deflection was precisely what it had been under the load of 1233 pounds. The beam had balanced at 911 pounds for about three minutes when the fracture took place. An assistant was sitting fifteen or twenty feet from the machine at the instant, but no one had approached the machine after the last adjustment of the weight.

This is a case without parallel in the experience of the writer, and its conclusion indicates a possibility of depreciation in resisting power of the class of metals of which tin has been taken as the type, which depreciation, in the present state of our knowledge of the properties of such metals in this regard, it may be safest to assume to be a source of danger in some cases in which the load approaches the maximum resisting power of the piece. This illustrates the case of progression of flow until the section most strained has been weakened to the point of actual molecular disruption, which disruption would seem to have been here produced by the effort of other and less injured portions, to resume their original positions, and to straighten the two halves of the bar. It would seem that such action should be determined by flow occurring in a somewhat ductile but still somewhat elastic metal.

The strain diagram of this bar is seen to be nearly hyperbolic; but the law of Hooke, *ut tensio sic vis,* holds good, as usual, up to a point at which the load is about one-half the maximum. The curve of times and loads, exhibits the rate of loss of effort while the bar was finally held at a deflection of 0.5456 inch, the load being carefully and regularly reduced, as the effort diminished, from 1233 to 911 pounds, at which

latter figure the bar broke. The curve is a very smooth one. The following is the record of the test:

BAR NO. 599.

90 parts zinc, 10 parts copper: 1 × 0. 992 × 22 inches.

LOAD;	INCHES.		LOAD;	INCHES.		LOAD.	INCHES.	
Pounds.	Deflection.	Set.	Pounds.	Deflection.	Set.	Pounds.	Deflection.	Set.
23	0.0033	......	363	0.0781	......	3	......	0.0336
43	0.0078	......	403	0.0881	......	643	0.1641	......
63	0.0127	......	3	......	0.0079	803	0.2149	......
103	0.0225	......	403	0.0886	......	1 003	0.3178	......
143	0.031	......	Resistance fell in 8 h. 30 m.			1 103	0.3921	......
163	0.0347	......	to 333	0.0886	......	1 203	0.481	......
Resistance fell in 15 h. 25 m.			3	......	0.0246	1 233	0.5209	......
to 143	0.0347	......	333	0.0896	......	Resistance fell in 15 m.		
3	......	0.0039	Resistance fell in 15 h.			to 1 137	0.5209	......
163	0.0391	......	to 302	0.0896	......	3	......	0.2736
203	0.0471	......	303	0.0876	......	1 137	0.5131	......
243	0.0544	......	403	0.1072	......	1 233	0.5456	......
283	0.0611	......	503	0.1282	......	......	......	......
323	0.0692	......	603	0.1521	......	......	......	......

The bar was left under strain at 11 h. 22 m. A. M., and the effort to restore itself measured, at intervals, as follows:

Hour.—11 h. 37 m.; 11 h. 50 m. A. M. 12 h. 2 m.; 12 h. 8 m.; 12 h. 25 m.; 12 h. 39½ m.; 12 h. 53½ m.; 12 h. 58½ m.; 1 h. 20 m. P. M.

Effort.—1 133; 1 093; 1 070; 1 063; 1 043; 1 023; 1 003; 993; 911 pounds.

At 1 h. 23 m. P. M. the bar broke.

An example of somewhat similar behavior, but exhibited by a metal of very different quality, is shown on the next page.

This bar was hard, brittle and elastic, but must apparently be classed with tin in its behavior under either continued or intermitted stress.

There seems to the writer to exist a distinction, illustrated in these cases, between that "flow" which is seen in these metals, and that, to which has been attributed the relief of internal stress and the elevation of the elastic limit by strain and with time.

This last phenomenon—the exaltation of the elastic limit by strain—has been observed very strikingly, by the writer, in the deflection of iron bars, by transverse stress. The plate exhibits the strain-diagrams

Bar 596.

75 parts zinc, 25 parts copper: Second casting: 0.985 × 0.985 × 22 inches.

Load;	Inches.		Load;	Inches.		Load;	Inches.	
Pounds.	Deflection.	Set.	Pounds.	Deflection.	Set.	Pounds.	Deflection.	Set.
23	0.0057	....	423	0.073	....	to 473	0.0866	....
63	0.0142	....	463	0.0799	....	3	....	0.0092
103	0.0207	....	503	0.0866	....	503	0.0894	....
143	0.0275	....	3	....	0.0014	543	0.0952	....
183	0.0346	....	503	0.0866	....	583	0.1012	....
223	0.0414	....	Resistance fell in 5 h.			603	0.1042	....
263	0.0485	....	to 489	0.0866	....	623	0.1075	....
303	0.0549	....	3	....	0.0074	643	0.1102	....
343	0.061	....	489	0.0866	....	663	0.1136	....
383	0.0669	....	Resistance fell in 18 h. 30 m.			Broke 5 sec. after with ringing sound.		

obtained by transverse deflection of 4 bars of ordinary merchant wrought iron which were all cut from the same rod. Of these, two were tested in the machine above described, in which the deflection remains constant when the machine is untouched while the load gradually decreased—or, more properly, while the effort of the bar to regain its original form, decreases. The other two were tested by dead loads—the load remaining constant while the deflection may vary when the apparatus is left to itself. (The record is given on pages 209–212.)

These two pairs of specimens were broken; one in each set by adding weight steadily until the end of the test, so as to give as little time for elevation of elastic limits as was possible, and one in each set by intermittent stress, observing sets, and the elevation of the elastic limit.

If the long-known effects of cold-hammering, cold-rolling and wire-drawing, in stiffening, strengthening and hardening some metals can be, as the writer is inclined to believe, attributed in part to this molecular change, as well as to simple condensation and closing up of cavities and pores, this exaltation of the elastic limit by distortion under externally applied force, has now been shown to occur in iron and in metals of that class in tension, torsion, compression and under transverse strain.

Referriug to the plate, it will be seen that there is exhibited the action in the latter case even more fully and strikingly than in the record above given, and a study of these typical examples cannot fail to prove both interesting and instructive.

Tests of wrought Iron Bars by Transverse Strain.

Samples 1 inch square, 28 inches long; 22 inches between supports.

No. 648. Tested in Fairbanks' Machine.

Load; pounds.	Inches. Deflection.	Inches. Set.
103	0.0132	....
203	0.0244	....
Resistance fell in 13 h. 35 m		
to 199	0.0244	....
303	0.0342	....
403	0.0428	....
Resistance fell in 1 h. 30 m.		
to 399	0.0428	....
3	....	0.0042
503	0.0528	....
603	0.0619	....
Resistance fell in 4 h.		
to 598	0.0619	....
803	0.0806	....
Resistance fell in 15 h. 15 m.		
to 789	0.0806	....
903	0.0907	....
1 003	0.0995	....
Resistance fell in 5 h. 20 m.		
to 987	0.0995	....
3	....	0.0049
1 203	0.1197	....
3	....	0.0071
1 203	0.121	....
Resistance fell in 2 h.		
to 1 187	0.121	....
3	....	0.0096
1 203	0.1226	....
1 243	0.1266	....
1 283	0.1301	....
1 323	0.1354	....
1 363	0.1421	....
1 403	0.1504	....
3	....	0.0196
1 403	0.1522	....
Resistance fell in 3 m.		
to 1 387	0.1522	....
Resistance fell in 2 h. 25 m.		
to 1 361	0.1522	....
Resistance fell in 39 h. 5 m.		
to 1 329	0.1522	....
3	....	0.0246
1 329	0.1451	....
1 403	0.1522	....
1 483	0.16	....
1 523	0.1647	....
1 563	0.1761	....
1 603	0.2548	....
3	....	0.1091
1 603	0.287	....
Resistance fell in 6 h. 3 m.		
to 1 457	0.287	....
3	....	0.1451
1 457	0.2863	....
1 603	0.2016	....
1 703	0.3921	....
3	....	0.2431
1 703	0.4301	....
Resistance fell in 20 h. 50 m.		
to 1 541	0.4301	....
3	....	0.2846
1 541	0.4296	....
1 603	0.4346	....
1 711	0.4456	....
1 753	0.4513	....
1 781	0.4651	....
Resistance fell in 6 h. 3 m.		
to 1 661	0.4651	....
3	....	0.31[illegible]6
1 675	0.4676	....
1 787	0.4808	....
1 811	0.5446	....
3	....	0.3771
1 811	0.5661	....
Resistance fell in 46 m.		
to 1 675	0.5661	....
Resistance fell in 17 h.		
to 1 661	0.5661	....
3	....	0.408[illegible]
1 661	0.5645	....
1 801	0.578	....
1 861	0.5886	....
1 877	0.6034	....
1 891	0.6626	....
3	....	0.4938
1 891	0.7001	....
Resistance fell in 10 s.		
to 1 801	0.7001	....
Resistance fell in 6 m.		
to 1 737	0.7001	....
Resistance fell in 5 h. 23 m.		
to 1 721	0.7001	....
3	....	0.5406

No. 648.—(*Continued.*)

LOAD;	Inches.		LOAD;	Inches.		LOAD;	Inches.	
pounds.	DEFLECTION.	SET.	pounds.	DEFLECTION.	SET.	pounds.	DEFLECTION.	SET.
1 741	0.7031	....	Resistance fell in 47 h. 37 m.			2 311	1.6466	....
1 911	0.7246	....	to 1 869	1.1316	....	2 341	1.6996	....
1 921	0.7566	....	3	....	0.958	2 351	1.7321	....
3	....	0.5746	1 941	1.1358	....	3	....	1.5196
1 921	0.7746	....	2 131	1.1551	....	2 351	2.0446	....
Resistance fell in 21 h. 48 m.			2 181	1.1686	....	Resistance fell in 16 h.		
to 1 767	0.7746	....	2 201	1.1981	....	to 2 135	2.0446	....
3	....	0.6028	2 211	1.2356	....	3	....	1.8441
1 767	0.7726	....	3	....	1.0361	2 135	2.0431	....
1 931	0.7876	....	Resistance fell in 9 h.			2 355	2.0646	....
1 995	0.8036	....	to 1 997	1.2356	....	2 391	2.0736	....
2 001	0.8266	....	3	....	1.1158	2 411	2.1136	....
3	....	0.6451	2 001	1.3016	....	3	....	1.8964
2 001	0.8498	....	2 211	1.3326	....	2 411	2.1451	....
Resistance fell in 21 h. 30 m.			2 231	1.3656	....	Resistance fell in 8 h. 35 m.		
to 1 831	0.8498	....	2 237	1.3936	....	to 2 238	2.1451	....
3	....	0.6780	3	....	1.1946	Gradually reduced strain.		
1 831	0.8471	....	2 241	1.4441	....	to 3*	....	1.9266
2 003	0.8641	....	Resistance fell in 13 h. 50 m.			Gradually increased strain.		
2 071	0.8819	....	to 2 041	1.4441	....	to 2 238*	2.1336	....
2 081	0.9396	....	3	....	1.2538	2 411	2.1516	....
3	....	0.7576	2 041	1.4421	....	2 491	2.1811	....
2 081	0.9886	....	2 241	1.4631	....	2 501	2.2121	....
Resistance fell in 21 h. 39 m.			2 281	1.4821	....	3	....	1.9886
to 1 871	0.9886	....	2 301	1.5216	....	2 501	2.2471	....
3	....	0.8148	2 311	1.5531	....	Resistance fell in 14 h. 10 m.		
1 911	0.9904	....	3	....	1.3436	to 2 295	2.2471	....
2 083	1.0106	....	2 311	1.6166	....	3	....	2.0331
2 121	1.0496	....	Resistance fell in 8 h. 8 m.			2 295	2.2456	....
2 131	1.0911	....	to 2 091	1.6166	....	2 541	2.2762	....
3	....	0.9021	3	....	1.4181	2 561	2.3102	....
2 131	1.1316	....	2 091	1.6166	....	3	....	2.0763

* Gradually reduced strain to 3 pounds, taking a number of readings; then gradually increased it to 2 238 pounds, taking readings corresponding to former ones; found that increase of deflection was proportional to increase of load.

No. 648.—(*Concluded.*)

LOAD.	INCHES.		LOAD.	INCHES.		LOAD.	INCHES.	
Pounds.	Deflection.	Set.	Pounds.	Deflection.	Set.	Pounds.	Deflection.	Set.
2 561	2.35	....	2 591	2.5247	....	Resistance fell in 61 h. 32 m.		
Resistance fell in 6 h. 18 m.			2 611	2.5334	....	to 2 363	2.8324	....
to 2 369	2.35	....	2 631	2.5927	....	3	....	2.5924
3	....	2.1402	3	....	2.3577	2 363	2.8286	....
2 369	2.3587	....	2 631	2.653	....	2 685	2.8627	....
2 551	2.3782	....	Resistance fell in 8 h. 2 m.			2 701	2.871	....
2 571	2.3854	....	to 2 371	2.653	....	2 710	2.8917	....
2 591	2.4287	....	3	....	2.4227	2 720	2.9297	....
3	....	2.1952	2 371	2.6532	....	3	....	2.647
2 591	2.5044	....	2 631	2.6833	....	2 720	2.9692	....
Resistance fell in 15 h. 4 m.			2 651	2.6992	....	Resistance fell in 5 h. 48 m.		
to 2 371	2.5044	....	2 661	2.7307	....	to 2 483	2.9692	....
3	....	2.2842	3	....	2.4987	3	....	2.7357
2 371	2.5022	....	2 661	2.8324	....	Bar removed: test ended.		

No. 649. TESTED IN FAIRBANKS' MACHINE.

LOAD. Pounds.	DEFLECTION. Inches.	LOAD. Pounds.	DEFLECTION. Inches.	LOAD. Pounds.	DEFLECTION. Inches.	LOAD. Pounds.	DEFLECTION Inches.
103	0.0139	900	0.0889	1 462	0.1505	In 2¾ m. was	0.3629
200	0.0238	1 000	0.0982	1 480	0.1569	1 620	0.3704
300	0.0328	1 100	0.1081	1 500	0.1619	1 640	0.3831
405	0.0425	1 200	0.1171	1 520	0.1709	In 6 m. was	0.4404
500	0.0519	1 300	0.1279	1 540	0.1804	1 660	0.4479
600	0.0602	1 400	0.1398	1 560	0.2078	1 680 (*b*)	0.4599
700	0.0689	1 420	0.1435	1 580	0.2429	.....	......
800	0.0787	1 442	0.1472	1 600(*a*)	0.2854	2 350	5.577

(*a*) At 1 600 pounds the beam sank instantly; ran the pressure screw down so as to keep the beam balanced for 2¾ m., with increase of deflection as noted. (*b*) At 1 680, ran pressure screw rapidly but steadily down, moving the poise along the beam to keep it balanced. The beam vibrated up and down, falling or rising instantly as the wheel was turned slower or faster. The resistance reached a maximum of 2 350 pounds, when the deflection was 5 577 inches.

No. 650. Tested by Dead Loads.

Load; Pounds.	Deflection; Inches.	Load; Pounds.	Deflection; Inches.	Load; Pounds.	Deflection; Inches.	Load; Pounds.	Deflection Inches.
100	0.015	400	0.0425	800	0.0858	1 400	0.1749
200	0.0229	600	0.0638	1 200	0.1456	1 500 (c)	0.2143

(c) At 1 626, the reading was not taken. Weights as follows were rapidly added, 4 or 5 pieces each minute, as follows:—82, 25, 42, 15, 16, 10, 15.5, 16, 25, 25, 25, 13, 11.5, 16, 27, 62, 40.5, 61, 45, 62 = 2 260.5 pounds. The bar sank rapidly, its side pressure splitting the wood which confined the mandrels. The set, measured after the bar was removed, was 2.5 inches. The total deflection is calculated as follows:—the elasticity of the bar remaining the same the increase of deflection over set is directly proportional to the load. (This is shown by the parallelism of the elasticity lines with the original line within the elastic limit.) Thus at 800 pounds, the set was inappreciable, deflection 0.0858; whence 800: 0.0858:: 2 260: 0.242 difference of deflection and set: set was 2.5, hence calculated deflection 2.742 inches.

No. 651. Tested by Dead Loads.

Load: Pounds.	Deflection; Inches.	Load; Pounds.	Deflection; Inches.	Load; Pounds.	Deflection; Inches.	Load; Pounds.	Deflection Inches.
100	0.0158	In 5 h. 46 m. In	0.6598	In 48 h. 30 m. was	1.9245	2 452	3.0732
200	0.0275	1 700	0.67	2 222	1.9379	2 484	3.0812
400	0.0489	In 3 m. was	0.6716	2 288	2.1386	In 39 h. 40 m.	4.2591
600	0.0709	" 16 h. "	0.7615	In 12 m. was	2.9535	In 43 h. 20 m.	4.2591
803	0.0913	1 800	0.771	2 266	2.9928	2 513	4.2623
1 000	0.1141	1 900	1.0904	In 17 m. was	3.0157	2 556	4.267
1 200	0.1394	In 3 h. 15 m. was	1.8567	" 3 h. 37 m. "	3.0236	In 4 h. 20 m.	4.2749
1 400	0.1701	" 45 h. 45 m. "	1.8709	2 288	3.029	2 589	4.2749
1 500	0.2465	2 005	1.8787	2 350	3.0426	In 48 h. was	4.6591
In 8 m. was	0.4307	In 3 h. was	1.8819	2 370	3.0433	In 61 h. 30 m "	4.6701
1 600	0.489	2 052	1.8886	In 25 h. 15 m. was	3.0677	Weights reached support. Test was ended.	
In 6 m. was	0.6504	2 115	1.8921	2 422	3.0701		

The strain-diagrams exhibited in the plate do not present to the eye one of the most important distinctions between the two classes of metals. As seen by study of these diagrams, both classes, when strained by flexure, gradually exhibit less and less effort to restore themselves to their original form.

In the case of the tin-class, this loss of straightening power seems often to continue indefinitely, and, as in one example here illustrated, even until fracture occurs.

With iron and the class of which that metal is typical, this reduction of effort becomes gradually less and less rapid, and finally reaches a limit after attaining which, the bar is found to have become strengthened, and the elastic limit to have become elevated. In this respect, the two classes are affected by time of strain, in precisely opposite ways.

The plate exhibits, even better than the record, the superior ultimate resistance of the bars which have been intermittently strained, as well as the elevation of the elastic limit. This parallelism of the "elasticity lines" obtained in taking sets, shows that the modulus of elasticity is unaffected by the causes of elevation of the elastic limit.

Evidence appealing directly to the senses has been presented in the course of experiment on the second class of metals, of the intra-molecular flow. When a bar of tin is bent, it emits while bending the peculiar crackling sound, familiarly known as the "cry of tin." This sound has not been observed hitherto, so far as the writer is aware, when a bar has been held flexed and perfectly still. In several cases recently, in experiments on flexure* of metals of the second class, bars held at a constant deflection have emitted such sounds hour after hour, while taking set and losing their power of restoration of shape.

During some of the experiments made, a very marked illustration of the decrease of set with time, which has been observed and described by Prof. W. A. Norton, has been noted, and the recovery of straightening power in the deflected bar has sometimes been strikingly large, amounting to nearly 30 pounds in 15 minutes. A record of one of these bars is,—

BAR No. 563.

17.5 parts copper, 82.5 parts tin. 0.986 × 0.993 × 22 inches.

LOAD;	Inches.		LOAD:	Inches.		LOAD;	Inches.	
pounds.	DEFLECTION.	SET.	pounds.	DEFLECTION.	SET.	pounds	DEFLECTION.	SET.
10	0.0027	......	140	0.0804	......	300	0.4597	......
20	0.007	......	180	0.1343	......	5	......	0.3084
40	0.0153	......	200	0.1666	......	Set decreased in		......
60	0.0256	......	5	......	0.0821	2 hrs. 20 min.		0.2845
80	0.0365	......	200	0.1798	......	to 300	0.5332	......
100	0.0499	......	240	0.2503	......	310	Bar broke in putting	
5	......	0.0092	280	0.3762	......	....	on strain.	

* Made in the mechanical Laboratory of the Stevens Institute of Technology.

After 300 pounds had been placed on the bar, and the reading taken, the screw was run back till the beam just balanced at 5 pounds, the pressure block attached to the screw just barely touching the bar. The set was then read, as above, 0.3084 inches, the beam slowly rising. The pressure screw was then run back till beam again balanced at 5 pounds, and the set measured 0.3022 inches. The time was 2 minutes. The beam again rose, poise on beam was pushed forward and balanced at 10 pounds; the time was 2 minutes. In 2 minutes more, beam balanced at 14 pounds. The pressure screw was again run back till beam balanced at 5 pounds and the set measured 0.0298 inches, The beam rose again; at 11 hours 37 minutes, A. M. In 2 minutes it balanced at 10 pounds, in 10 minutes at 16 pounds, and in 29 minutes at 23 pounds. The beam was again balanced at 5 pounds, set measured 0.2902 inches. The beam rose in 4 minutes. In 29 minutes the beam balanced at 14 pounds, and in 65 minutes more it balanced at 20 pounds. The beam was again balanced at 5 pounds and the set measured 0.2845 inches. The total decrease of set in 2 hours 20 minutes was 0.3084 — 0.2845 = 0.0239 inches. Then replaced 300 pounds, and read deflection 0.5332 inches; increased the pressure, but the bar broke before 310 pounds was reached.

ERRATA:—On page 7, line 8 from bottom, for "remit" read "re-unite"; page 10, line 7 from top, for "crevices" read "crevasses"; page 17, line 8, for "1865" read "1862"; page 22, line 5, for "Tausse" read "Fausse"; line 6, for "Raccomci" read "Raccourci".

On page 107, line 4 from bottom, for "14 000" read "1 400".

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NOTE.—This Society is not responsible, as a body, for the facts and opinions advanced in any of its publications.

CXXIV.

ON RAILROAD ACCOUNTS AND RETURNS.

A Paper by WILLIAM P. SHINN, Member of the Society.

Presented June 2d, 1876.

In the report of the Railroad Commissioners of Massachusetts for 1875 as well as in previous reports, special and needed mention is made of the great defects in the keeping of accounts by railroad companies, and of the total want of reliability of returns based on accounts so miscellaneously kept.

Having had a large experience in railroad accounts, and from a personal investigation of the affairs of many railroad companies, having learned that lack of any system of accounts worthy of the name is the rule, and its existence the exception (particularly on the old and long established roads), I beg to offer the following suggestions.

As to *Accounts*—they being the basis from which returns are to be made, and the returns being worthless unless accounts are correctly kept, it would seem to logically follow, that if the State is authorized to require *returns*, and does require them, it should also require that certain general principles should be observed in keeping the *accounts* from which returns are to be made, and on the correctness of which returns depend.

I suggest as proper requirements in the keeping of *accounts* that—

1°. *Earnings* should be shown and reported rather than *receipts*, as a large proportion of each month's "earnings" only become "receipts" in the following months.

2°. *Expenses* should include all liabilities incurred for services *rendered* and materials *used* in the current month, and other liabilities liquidated in amount, regardless of when they are paid or payable.

3°. *Maintenance* should include the cost of labor and materials expended, such as may be necessary to make and keep the track, machinery and structures "as good as new," less the *market value* of old materials released from use.

4°. *Construction* should include only actual *addition in extent* to tracks and structures, real estate, &c.

5°. *Equipment* should include only actual *additions in number* to engines, coaches, cars, &c., the original number being kept filled by rebuilding, &c.

6°. *Betterments* should consist of the actual difference in value, between *improved* track, structures or equipment and the cost of replacing those worn out, with others of same quality as were originally constructed—such as steel rails in place of iron, brick or stone buildings in lieu of wood, &c.

7°. *Liabilities* should include all expenses incurred and construction, betterment and interest liabilities, which may be *unpaid* at date of report.

8°. *Assets* should include all earnings or income *earned*, but not *collected* or become receipts at date of report.

9°. *Interest* should include all interest due or past due, whether paid or unpaid at date of report.

10°. *Cost of operating* should be made up from *expenses* and exclusive of all *betterment* expenditure.

Accounts so kept, and in which the companies were required to report the specific tracks, structures, &c., charged to construction and equipment, and the items for which betterments were charged, would be incapable of manipulation in the manner and to the extent set forth in the Massachusetts report, and *returns* based thereon would give a volume of information now sought for in vain in State reports. The system for keeping them is now in operation on several leading lines in the West, and it is as simple as it is comprehensive.

As to *returns*—the careful investigator into railroad economy will look in vain through all the volumes of State reports to find data upon which to base any calculations as to actual cost of traffic or the economy or otherwise of its transportation, for the following (as well as other) reasons, viz.:

Most railroads have a traffic largely preponderating in one direction—much of the traffic transported in that direction must pay (if it is to realize a profit) not only the cost of its transportation, but also that of returning an empty car.

Nothing bearing upon this fact appears in any of the State reports, so far as I am aware, and nothing is more absolutely necessary to a correct understanding of the business of a railroad and of the economy with which its traffic is moved.

I therefore suggest as additional items to be embraced in the returns:

1. Mileage of *loaded* cars in each direction.
2. " " *empty* " " " "
3. Ton mileage of freight " " "
4. " " " passing over whole road in each direction.
5. Earnings from freight in each direction.
6. Earnings from passengers in each direction.
7. Passenger mileage " " "
8. Terminal expense chargeable to freight.
9. " " " passengers.

The above are the most important items of information omitted, although there are others which would be desirable, but not so absolutely necessary.

But the requirements and forms of returns do not agree in any two States, and this fact, while it baffles the investigator who seeks to compare the results of operating roads located in different States, also causes railroad companies whose roads extend through or into several States much trouble in rendering their returns, particularly as they are required to be rendered to different dates, thus: Ohio report is to June 30th; Pennsylvania report is to November 1st; Illinois, New York and Massachusetts each to September 30th.

Most railroad companies end their fiscal year at December 31st, but as Legislatures generally meet in December or January, returns cannot be made to December 31st, tabulated, and reports prepared in time to lay before the Legislature, hence the diversity in dates.

As railroad companies require two months at least to close their accounts and make up their returns, and should have three, the most satisfactory date to which returns should be made would be June 30th, giving the companies until October 1st to make them, then the commissioner or other State officer would have two to three months before the Legislature meets, in which to tabulate, review and digest.

That these reforms would meet the views of the accounting officers of the railroad companies, I fully believe, and to accomplish them, co-operation between the various State Commissioners is required.

To bring this about, I would suggest the appointment of a committee of the Society on "uniform Accounts and Returns of railroad companies to State Commissioners," with authority to correspond with the Commissioners of the several States, and to arrange for a meeting with them to consider the steps necessary to the adoption of this desirable improvement.

I have before taken occasion to state to the Society, and I now repeat it as my view, that the railroad companies of the country may be relied on to give to the people of the country the advantage in lower rates of transportation, of all improvements made and economies effected, and it is to investigations scientifically made on data known to be reliable that we are to look for such improvements and economies, hence the importance of establishing the State returns on a correct basis.

ERRATA:—On page 168, bottom line, for "γ" read "δ," and next line above, for "ψ" read "δ"; page 169, line 4 from top, for "γ" read "δ"; page 178, line 7 from bottom, insert "feet" after "post"; and page 185, line 4 from bottom, for "shape" read "shops."

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DISCUSSIONS OF SUBJECTS PRESENTED AT THE EIGHTH ANNUAL CONVENTION.

ON THE THEORY OF CONTINUOUS GIRDERS IN RELATION TO ECONOMY IN BRIDGE BUILDING.*

MR. CLEMENS HERSCHEL.—In a paper on swing or draw bridges† which I presented to the Society in September, 1874, I agreed to the general conclusion of the paper now under discussion—that it is inexpedient to build continuous fixed spans. The motto of my paper, giving the factors which make up a reliable investigation, leads the way to latter statements in the same paper, that these factors have not been employed in investigating this subject. If, after arriving at this conclusion, I then submitted‡ an application of the theory of the elastic line, with new developments, and some easy and simple methods of practical use, it was, because that as yet there is nothing better for the calculation of strains in continuous swing bridges. Grant that in certain cases of load, or deformation of girder, strains computed by this method are 10 per cent. in error, what better can be done; in my judgment, the paper under discussion affords no relief.

Referring to page 193, to me, it seems rash and impracticable to construct a swing bridge of any size as there sketched; such a bridge would be in unstable equilibrium if symmetrically loaded—the most favorable case—and if unsymmetrically loaded, it would come to grief. However, I think the solution of the difficulty lies in the direction of the sugges-

* Referring to—CXXII, Application of the Theory of Continuous Girders to Economy in Bridge Building; C. Bender, page 147.

† CV, On the Principles of the Construction of Revolving Draw Bridges. Vol. III, page 395.

‡ Vol. III, page 395.

tion there made by the writer. Continuous bridges, built with hinges, will escape the uncertainties caused by defects of theory and by inequality of heights of support;* continuous girders and elastic arches, can both be constructed so as not to be affected by the circumstances inherent to an application of the theory of the elastic line.

I care little about priority in matters of science; often the same thing is invented at one or at different times by several persons; but as I have publicly claimed to have made some improvements in the consideration of continuous girders, a few words on that, may not be out of place. In these matters, to print or publicly describe the novelty claimed, fixes the date of its invention. Will the writer state, where the apparatus mentioned on page 163 was described, or where an account of the mounting of the Silesian Bridge is given? I have looked in vain in English, German or French books and journals for anything of the sort. Since my paper was written, I have seen an account of the actual weighing off of reactions of continuous model beams. Referring to what is said of Prof. Sternberg,† although I was instructed by him for three years, I know that in 1861 he did not teach what with much labor I evolved from the unexplored regions of common sense and science in 1874.

The calculations of strains in draw bridges are not more complicated, delusive or untrustworthy than those for properly constructed continuous fixed bridges, and further, one of the greatest uncertainties in the calculation of continuous fixed bridges, the effect produced by an uncertain settlement of the points of support, is in the case of draw bridges, when properly constructed, entirely removed; I have treated this matter fully in my paper, and will here only remark that should any of the three or four points of support settle or raise, it requires but an adjustment of the end supports, to bring all of them to the same relative position again.‡ Hence it is only the strains due to the moving load about which there can be the slightest doubt, under so far accepted methods of calculation.

The great and unnecessary complication, spoken of in this paper, does not appear to me. I know a case where the strain sheet for a draw was furnished for $40; this is a good test of the complicity, though the

* That thereby they escape the uncertainty of strain, caused by inequality of moduli, by several systems of diagonals, and (with a single exception) the additional strains due to unequal heating of the chords, I must deny; simple fixed girders, whether link or rivetted, are affected by all of these causes. † Page 192.

‡ It may be mentioned, draw bridges can always be so arranged, that when unloaded, their whole weight rests only on the centre pier.

case was exceptional, being a second calculation, made on account of changes in the general shape, panel lengths, &c.

I have said, the effect of an unequal expansion of the two chords, from difference in the moduli of the several members of the bridge and from several systems of diagonals, is felt by single fixed spans also; is it true that a fixed bridge will remain unaffected by difference of temperature in the two chords? A bridge, with two or more panel systems, however, will have its strains disturbed, but it is a small matter and is felt less by a single than by continuous spans. I admit, that the effect of different moduli of the several members of a bridge, is the old question, in another shape, of whether it is advisable to have cast and wrought iron in one and the same structure, and the query arises, why cast iron and wrought iron are not worse than two varying specimens of wrought iron? We always try to get materials in a structure, of as uniform a quality as we can, and that seems to be all that can be practically done in the matter. No harm, on account of varying moduli, has yet resulted anywhere, that I am aware of. The greatest weight is to be laid upon the behavior of a structure during 25 or 30 years of use; here we have the *experimentum crucis*, besides which all other experiments sink into insignificance.

In the case of two or more panel systems, if more than one diagonal reaches the point of support, the distribution of the reaction must necessarily be indeterminate between these several diagonals. I could quote from text books, that it is only an allowable assumption to calculate such bridges, by first resolving the web into its primary systems and taking up each one separately; there must be some indeterminateness in all such bridges, but it is so small that, to my mind, it may be neglected in the case of continuous bridges, as well as in fixed spans. I have mentioned these points, not for the value they have in actual practice, but because it seems to me that they belong, if anywhere, to a consideration of *all* bridges, and not in an arraignment of continuous bridges alone.

I must take exception to a bit of iconoclasm,* whereby Leonard Euler is made the father of the general theory of the elastic line, thus displacing Navier from the place generally ascribed to him. If we are to go back to the beginning of things, why not Bernouilli, in 1705, or Mariotte, in 1680, instead of Euler; and if the first writing out of the present theory of the elastic line is to decide, I would propose Eytelwein instead of Navier. The paper upon which Euler's fame as a writer on

* Page 149.

continuous elasticity seems to depend, is in Transactions* of the Petersburgh Academy, 1773. Eytelwein, in Transactions† of the Berlin Academy, 1804-11, read January 10, 1805, expressly says, that neither Euler nor D'Alembert, who had written on the subject, have succeeded in determining the pressure which a beam, resting on more than two supports, produces on these several supports, and then he goes on and does it, giving the now generally accepted fundamental equation for determining these pressures and other results of the problem and makes experiments on deflection, &c. which prove that he is about right. If Navier is to be displaced from the high position he has held in this connection, I think it should be in favor of that German—Navier and D'Aubisson combined, who wrote some 20 years before Navier—the well known Eytelwein. Again, why Bertot instead of Clapeyron? It is true that Bertot first developed a part of the theorem ascribed to Clapeyron; nevertheless, the labors of the latter were not without novelty, the so-called Clapeyronian numbers, for example; he first applied the method known by his name, to the calculation of a bridge, and to this day receives among all writers in France, the credit of the improved method of calculating continuous girders, where they ought to be, if they are not, well acquainted with the facts, no less than elsewhere.

As was said at the outset, I agree perfectly with the writer of the paper that it is inexpedient at this stage of our knowledge upon bridge building, to construct continuous fixed bridges; but I do not agree with him, that a continuous draw bridge is an entirely worthless affair; until somebody invents something better, we must go on and build them as well as we know how.

Other points of disagreement have been discussed above; the mathematical part of the paper, a new method of calculating continuous girders, I have not examined, with the care it demands, in order to express opinion upon it. However, the comparison of two fixed and two continuous spans of a certain length does not necessarily tend to correct results. With a change in the proportion of dead to moving weight, an entirely different result may be arrived at.

Mr. A. Jay Du Bois.—In order to determine the actual strains either in a "simple" or continuous girder we need only to calculate the movements and reactions of the supports and then the strains may be found correctly. So it comes down to that which can be very easily tested by experiment. We have formulæ for movements and reactions for any number of supports and lengths of spans. Are they correct, or are they

* Page 289. † Pages 28-64.

in practice sufficiently correct? This is easily decided by a few experiments. If found correct, it seems to me the theoretical part of the question is easily disposed of. Other things being equal, we can properly take the results for two similar cases and compare them. These results hold good at least for other spans of same length and dimensions. Such comparison shows a theoretical gain of 30 per cent. and over, for the continuous girder. Of course, it is not claimed that the continuous girder is in all cases to be preferred, but only that in some cases, theory indicates a decided gain. Now then, evidently the first thing is to decide by experiment how much confidence we may place in our theoretical results. This, as we have said, is easily done. We have only to test the reactions as given by formulae. The reactions deduced from theory of flexure for beams fixed at one end and supported at the other, &c. as given in text books, are found to be practically correct. The same theory applies to the continuous girder, and if the reactions are correctly given the strains admit of no question.

Admitting now the theoretical gain, the question practically is, how much of that gain can we actually realize? Upon this it seems to me that the whole question turns, and who without trying can decide how much of this gain can be utilized and how much must be lost? Who can pretend, without actually trying, to estimate the precise amount which should be deducted from this gain? These are questions which can be answered only by intelligent and progressive practice. Difficulties suggest themselves to almost every project. A tried and universally accepted theory points to certain conclusions. Would it not be well to try how nearly the conclusions of theory hold true in practice before deciding this question.

We do not need to ascertain the actual value of the deflection in order to know what the reactions are. The writer makes of this a strong point, but as a matter of fact it matters not how near absolutely we can calculate the deflection. The results of theory are based upon relative not actual values of the deflection. But even if it were not, it by no means follows that the same proportionate error is made in the reactions as in the deflection. It is upon the first that our calculation depends, and an error even of 90 per cent. in the second proves necessarily nothing. In reality, the actual deflection is not considered, and it is a matter of perfect indifference whether we can calculate it correctly or not.

Again, the greater the number of spans, the more marked the influence of continuity. In the example given in the paper we have two end spans only, and more than half the benefit of continuity is lost. For

the same length of span (200 feet), fixed horizontally at the ends, the saving as indicated by theory is 50 per cent. This is evidently the limit of saving for that span, since the more spans the nearer it approaches to fixity at the ends, and such a result is worth a little serious consideration at the hands of the practical engineer. The comparison is scarcely then a fair one. If we conceive a beam continuous over a number of supports, and then suppose the same beam cut in two over each support, there would, I think, be little question as to which is the strongest and stiffest. If I understand the conclusions of the writer, he would give the preference to the second construction.

The whole question resolves itself into this: theory indicates a decided gain; ought we to test this by actual trial or hastily decide that theory is at fault and there is no use trying; especially when the theory is a well received one, and one which has thus far been found practically valuable, and when the indicated gain is an exceedingly large one, and when finally we risk nothing by trial. It seems to me that the continuous girder has its proper place among bridge systems, and that in its place it is superior to the simple girder.

The closing suggestion that continuous girders should be built with hinges, reminds me of Alexander the Great's solution of the Gordian knot. If the chords are cut, continuity is destroyed and the advantages of continuity lost. It makes no difference where the hinges are inserted, whether at the ends as in simple girders, or at points of inflection; in any case you lose the advantage of continuity.

Mr. Charles Bender.—With a desire to bring about a fair discussion of the subject of continuous girders, I laid before the Society, the result of studies beginning in 1867 and extending over four years. What I thought worth while to thus present has been as carefully worked out as was possible for me. The result of these studies was previously communicated to the Society of German Engineers of Berlin during the summer of 1870, which, however, was not fully printed until 1873.* Since 1872, it was my intention to bring before the Society the subject of continuous girders, but I only found time this year so to do.

The views briefly laid down in the two articles, referred to the use of scales in adjusting the reactions of continuous girders, the importance of considering the deflections due to the webs of continuous girders, and especially to the impossibility of counting on a constant modulus of elasticity. In each, it was also stated that the expectation of economy from

* The same, in an English translation was published in the *Railroad Gazette* of New York, in 1874.

continuous skeleton structures is chimerical for railroad spans even as large as 400 feet. These ante-date Mr. Herschel's paper on "Revolving Draw-Bridges," and expressed in 1870 nearly the contrary of what he in 1875, held to be proper principles.

In the consideration of this subject, there are three phases through which an engineer must pass. The scholar just from school admires continuous girders unconditionally and believes in savings of from 30 to 50 per cent. The next period is that of one who thinks the theory might conditionally be made useful by improving continuous girders. This standpoint was occupied by me from 1866 to 1870, when I worked out a paper* recommending the use of pin joints and of hydraulic scales for regulation of reactions, and introducing the correction of the theory due to the web systems. But in 1870, I was obliged to give up this position. The third phase is the one which I have laid out in the paper now under discussion. Having, since 1870, fully recognized the valuable features in economy and as to science of the better class of American structures, I totally abandoned the hope of attaining advantages by adoption of the principle of continuity, unless according to Koepke's invention, for very large spans (of limited depth) the principle of hinges was introduced, which would exclude from the calculation all those uncertainties dwelled on, in my paper.

The first step which I took (in 1867) towards realizing the idea† of weighing the reactions, was to place on one or more piers, systems of springs, which having been previously tested and made adjustable would by their deflections permit the reactions to be adjusted and measured as closely as required by practice; also by their deflections the moments under a moving load would be reduced.‡ Having found in the same year, that neither by this method nor by mere lowering of center bearings a saving can be secured, I turned my attention toward a cheap apparatus on the principle of the hydraulic press combined with weighing levers, which, designed in detail, was submitted to Prof. Sternberg for his opinion ; he approved of it in general terms, but added that the plan had already been carried out on a bridge in Silesia.§ Continuous girders in North Germany having lost most of their charms, experiments in this direction, from which some here still expect great success, were with good reason, not continued.

* Which was then submitted to Mr. C. Shaler Smith, Member of the Society.

† The mere idea is old, and was given in the lectures of Prof. Sternberg.

‡ This idea at one time I held to be so valuable that I prepared a drawing towards having it patented ; this with signatures of witnesses is still in my possession.

§ His letter was received in 1870.

Continuous girders have been unfavorably criticised by early writers because—*first*, of the uncertainty of the heights of support and consequently of moments and shearing forces; *second*, of the great change of pressure and tension in one and the same members; *third*, the unreliability of the supposition of a constant value of modulus.*

The shortcomings of the theory are believed to have been proved by me. *First*, the modulus by other writers supposed to be a constant value, or nearly a constant value, is a very indefinite quantity;† *second*, the common theory of continuous beams cannot be applied to deep skeleton structures without considering the compressions of posts and the extensions of the ties of the webs;‡ *third*, unequal heating of chords and strains in some points, of over 50 per cent. of those calculated under the usual theory;§ *fourth*, in case of two systems of diagonals and posts in a continuous girder none of the strains can be calculated, they can only be guessed at; *fifth*, the members of continuous girders suffering pressure as well as tension, must be proportioned to carry the sum of both, but not only the maximum thereof; *sixth*, continuous girders (up to 400 feet railroad spans, at least) if properly designed, do not give any economy over single spans of proper proportions; because—(*a*), continuous trusses require heavier webs than single span trusses (as far as known, first mentioned, but not sufficiently appreciated, by Prof. Kulman in Zurich:** (*b*), continuous girders require the use of a class of masonry which is more costly than would be necessary for single spans;†† and (*c*), continuous trusses require more careful workmanship than do single spans, and rolling them over the piers is no longer the preferred best method of putting them up.‡‡

The paper on "Revolving Draw-Bridges," to which reference has been made§§ substantially presents the theory and enumerates the faults of continuous girders, as generally found in text-books. It has been worked out with great care, and I admit that the writer has almost exhausted the application of the usual theory to the subject. The subject of draw bridges, however, was treated in substantially the same manner by Prof. Steinberg in 1857, in his lectures (without algebraical ballast); it was again shortly treated by Messrs. Chanute and Morison in their

* This was mentioned as a disturbing element in the theory of continuity by Mr. Baker, of London, at the same time as done so by me, (1870). † Pages 156-161.

‡ Paper in *Railroad Gazette*, 1874; also page 198. § Pages 163, 186, 188.

** His Graphical Statics, 1866, page 542. †† Pages 163, 191.

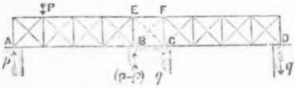
‡‡ Report of the Northeast R. R. of Switzerland, page 29, shown at Philadelphia Exhibition.

§§ CV. On the Principles of Construction of revolving Draw Bridges, C. Herschel, Vol. IV., page 395.

work on the Kansas City Bridge, and was developed in regard to draw bridges just touching the supports when unloaded, by another Member of the Society, Mr. C. Shaler Smith. These theories were applied before the appearance of the paper referred to, in quite a number of well-designed and well-working draw bridges in this country, of such huge dimensions as cannot be seen elsewhere.

I will now briefly refute the objections made to conclusions arrived at in my paper.

1°. A swing bridge with two middle posts, but without diagonals between them, when in position, operates as a continuous girder in fully stable equilibrium. The bridge rests on four supports, A, B, C, D.



Any unsymmetrical load, P, is taken up by the piers A and B. The reactions at A and B, of course are modified by the principle of continuity, but their sum is equal to P. The continuity calls forth a force—q, neutralized by a force $+q$ at C. The couple $-q$, $+q$ creates the moment which brings tension in the chord EF, and presses into BC. Now, if there are applied no diagonals, EC, BF, whether the supports B and C be elastic or non-elastic, the bearings B and C must take up $(P-p)$ and $+q$, and since there is no shearing force in the trusses between BE and CF, no diagonal is required when the draw bridge is in position.

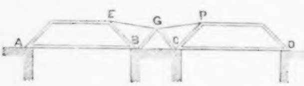
Since further, the table is always elastic or moveable, more or less, and in an unknown degree, the arrangement without diagonals theoretically would be desirable. Practically, it will be well to come as near to this modification as possible.* The prophecy, that a draw bridge without these centre diagonals would come to grief, under unsymmetrical loadings, does not harmonize with the science of mechanics nor with practical fact.

2°. It has been said, that for want of anything better, continuous draws must be studied and built. This conclusion should be modified. There is already a better plan. Sever entirely the two spans of a draw when in position, and connect them when to be moved, and you have a much better draw bridge than the one constructed on the doubtful theory of continuity. Singularly enough, this idea has been studied at the same time by the Keystone Bridge Co. and by myself.

* This conclusion follows directly from what was said on pages 151, 152. A Member, Mr. Alfred P. Boller, designed and executed a draw of this kind, which has no center diagonals at all; (roadway bridge at Troy, N. Y., with draw-span, 258 feet). It swings around also without diagonals in the center, though, as he remarks, it sways a little more than he would wish, probably because it bears completely on the center, so as to leave some play over the wheels.

I am informed that this Company has designed and made proposals on draw bridges on this plan. The central top pins pass through oblong holes, the centre diagonals are connected with separate top-chord pins between and near to the main pins. The draw in place is raised by means of hydraulic presses placed underneath the ends of the trusses (at A and D) and operated from the center. Before turning the draw, its ends are lowered, when the main pins come to bearing at E and F, and the diagonals are brought into action.

My system differs in details.* Two spans AB and CD are connected by chord-bars EG, GP, and by bottom chord, BC. The point G can be connected with B and C by adjustable members BG, CG. When the draw is in place, the members are disconnected, and EG, GP, form nearly a straight line of so much deflection as to prevent their coming into tension when the single spans are loaded. By screwing up BG and CG, the bars EG and GP can be brought to a powerful tension so as to lift the ends A and D, to aid in turning the bridge. The mechanical labor may be reduced by an apparatus such as used by Herr Schwedler† or on the plan exhibited in Vienna by Van Hasselt of Holland. Instead of the top-chords being adjustable, each bottom chord BC may be cut into two pieces, between which hydraulic presses can be inserted. I have calculated the strains and sections of such bridges, and found them at least as economical as continuous draw bridges.



3°. It is asked why the calculation of continuous draws should be less trustworthy than that of fixed continuous spans. I answer: (*a.*) Because the compressibility of everything below the trusses at the center pier is very uncertain. Mr. Herschel neglects this point altogether.‡ (*b.*) Draws with adjustable ends, especially, are liable to variations of strains, and even the introduction of adjusting rams or any other weighing apparatus will lead to variations, because the intention is likely not to be properly carried out. (*c.*) The masonry of round piers often is bad in the center, and center-bearing tables should therefore be founded on extra good masonry. (*d.*) Draw bridges with non-parallel chords (like

* It was first conceived by me about two years ago, and was explained to Mr. Alfred Boller, over a year ago. † Erbkam's Bauzeitung, 1871.

‡ In his paper on Revolving Draw Bridges (CXV. Vol. IV., page 432), he also neglects the deflection of the middle beam BAC; this deflection is material, and causes an error twice as great in the equation $C_2 = -C_3$ which for a beam of 24 feet length, under usual strains, is from ^{1} to over $\frac{4}{8}$ inch, according to construction.

that of Mr. Herschel), if calculated under the common theory of continuity, are not as properly proportioned as if in accordance with theory the chords had been parallel. The usual formulæ, however, are also applied in this instance, and considering the large factor of safety, the supposition of extreme live loads and many other arbitrary suppositions, this one may be accepted. In a similar case (a two track draw of 200 feet length, single web system), I have added 10 per cent. to the sections over and near the round pier. Building draws without end-pressures when unloaded, has been done, first by Mr. C. Shaler Smith, and after him by others (also by myself), before the paper on "Revolving Draw Bridges" was written.

4°. The writer of that paper declines to admit that the "great complicity of calculation" of draw bridges with over 3 continuous spans, should be dispensed with. Our views in this regard are diametrically opposed. Where he uses, in calculation, several pages of formulæ, I do not need any.

5°. In the discussion it has been asserted, that unequal heating of the chords of single spans and of continuous girders, produces similar results. Single span bridges with one or more intersections of diagonals, so long as they are provided with hinged joints, and if without counter diagonals, are altogether free from such influences. Trusses with counter rods as usually applied in quadrangular trusses, however are affected by unequal heating of the chords; with a difference of 30° Fahr. in temperature of chords, the counters will receive extra strains of about 2 000 pounds per square inch, or else be relieved of their original tension. But since the counters and adjacent parts are made stronger than indicated by strain sheet, this influence is immaterial, and does not lead to any appreciable error in the chords, posts or main ties.

6°. Mr. Herschel is not very fortunate in his historical statements. In his paper, he incorrectly says,* that "Navier, about 1830, first propounded the theory of the elastic line," and again that Clapeyron, in 1857, reduced these calculations for the first time by introducing the relation between each three consecutive moments over the piers. I said† that the general theory of the elastic line was first given by Leonard Euler, and the other relation by H. Bertot previous to Clapeyron. These corrections are verbally true, and are based on historical facts. Leonard Euler, in 1744 wrote a book,‡ in which the elastic curves are treated, and the crowning triumph of this great, if not greatest, analytical

* Vol. IV, page 396. † Page 149. ‡ De Curvis Elasticis, Lausanne and Geneva, page 250, etc.

mathematician of the last century, was the very equation which Mr. Herschel ascribes to Navier; that the moment of exterior forces for any point is proportional, for flat curves, to the second differential coefficient. Euler showed that the circle, the parabola, the parabolic curves and many others are to be considered as elastic lines. This general law given in an integrable form, in fact there remains little more but common algebraic labor, towards applying it to all questions of continuity.

Euler further, in 1757,* applied the same law to the strength of pillars (a problem of difficulty leading to exponential functions); for this he is quoted in England as the "immortal" Euler. He also applied his theory to the difficult problem of the pressure on the four legs of a table. P. S. Girard† in 1798, treats the subject of a beam *encastree* at both ends which, as well known, is the same problem as that of continuous girders, so that he at least would antedate Eytelwein. But I have only asserted, that Euler is the originator of the general theory of the elastic line, so that giving this honor to Eytelwein, after first having quoted Navier, is not in place.

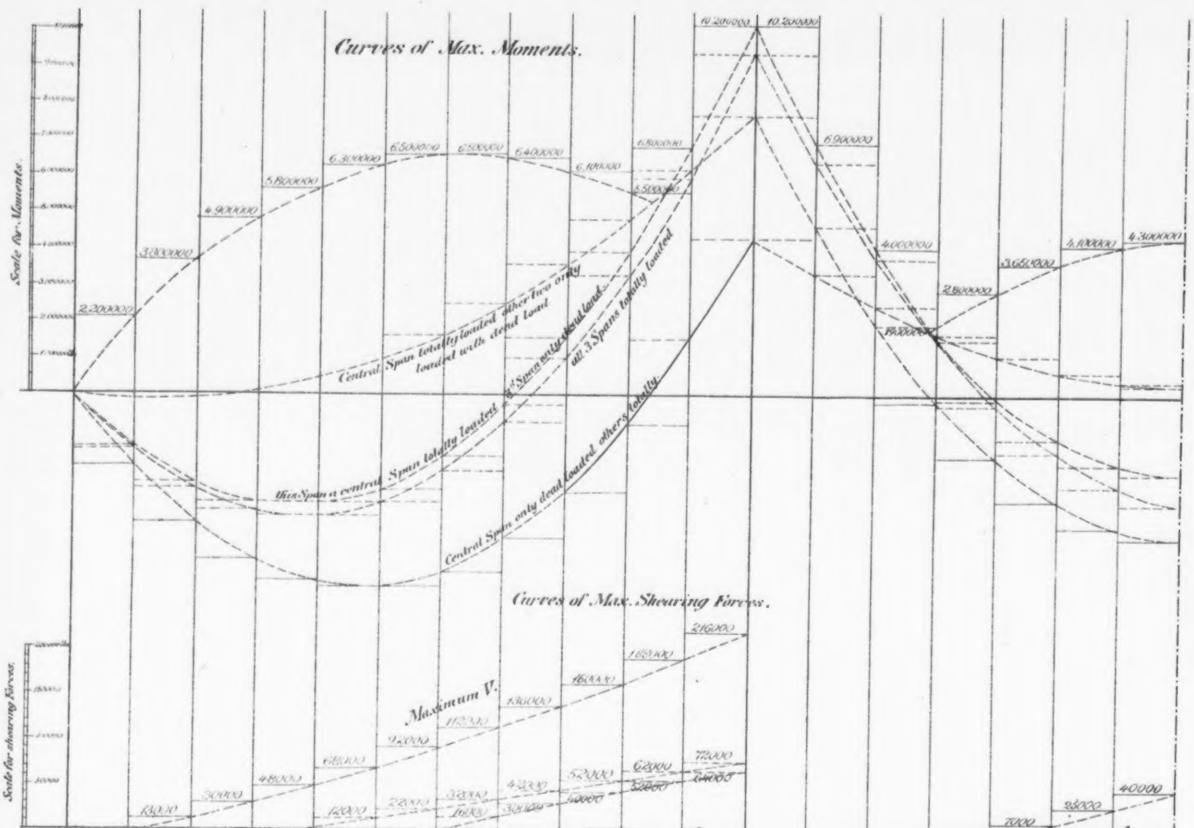
I am asked, why not give the credit to Mariotte or to Bernouilli? The answer is not difficult. To Mariotte (or to Hooke, as the English books quote), we owe the notion of the extensibility of the fibres of all bodies being proportional to the forces, whilst Galileo had supposed the fibres to be inextensible. To Jacob Bernouilli (1695), we owe the introduction of compressibility and therewith the introduction of the neutral line; whereas Galileo, Mariotte and still Leibnitz had supposed the fulcrum of the interior forces of a bent body to be in its lower or upper surface.

Jacob Bernouilli, the elder, just before his death, tried to solve the problem of the elastic line (1705), but did not succeed. The progress in the integral calculus, invented by John Bernouilli and highly furthered by Euler, only rendered it possible for the latter to solve this problem in its full generalisation, and to apply it with La Grange, D'Alembert, Daniel Bernouilli and others to the vibrations of elastic rods, plates, &c., which are problems of great difficulty, involving the highest mathematics.

I have mentioned that Girard treated the proposition of finding the reactions and the elastic line of a beam *encastree* at both ends. After him Cauchy and Poisson worked out still more the theory of elasticity, so that

* Memoires de l'Academie de Berlin.

† Member of the French Institute, in his work "Traite Analytique de la Resistance des Solides," &c., and beginning at page 50, he translates Euler's "De Curvis Elasticis," where his theory can be studied in French.



Curves of Max. Moments.
Scale for Moments.
2.200000
3.800000
4.900000
5.800000
6.300000
6.500000
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6.100000
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5.500000
10.200000
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6.900000
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4.300000
Central Span totally loaded other two only loaded with dead load
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Central Span only dead loaded others totally
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Scale for shearing Forces.
Maximum V.
13000
30000
48000
68000
92000
112000
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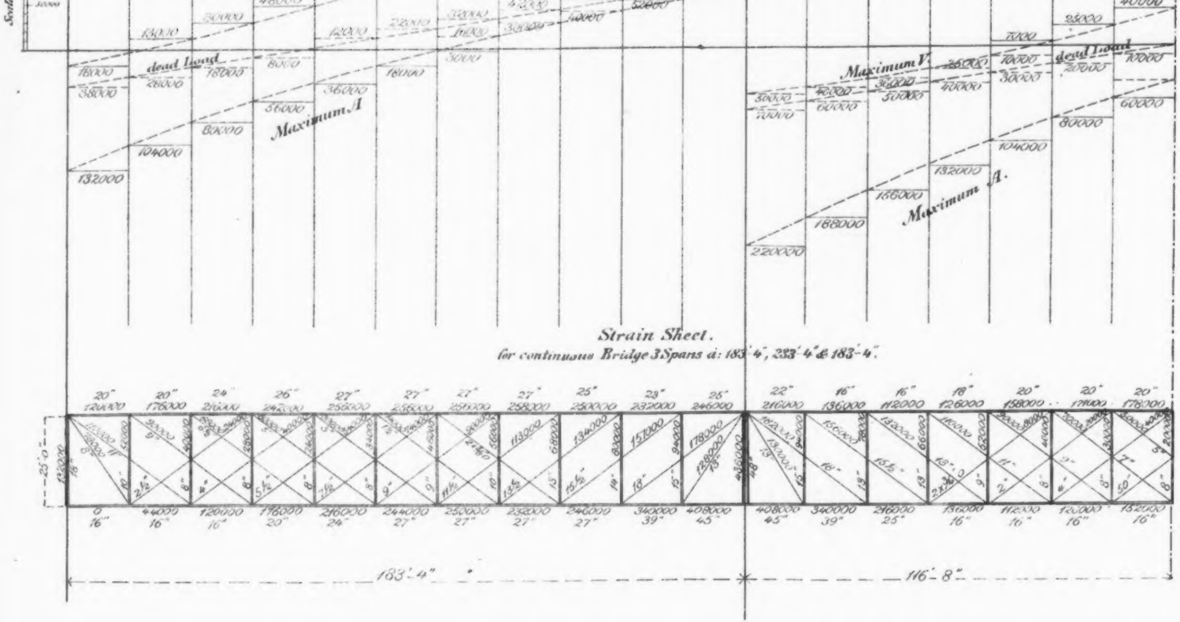

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for Navier there remained only the task of presenting Euler's theory in the elegant form, and of applying it in such a manner, as to make it attractive and acceptable to engineers.*

Regarding the priority of H. Bertot or of Clapeyron, it is sufficient to remark, that a mathematical relation found, is the property of the discoverer. The honor belongs to Bertot to have first given the relation known by Clapeyron's name; that Clapeyron extended the results from this relation (Clapeyron's numbers) has only remote interest, even for theorists. The practical value of this extension is small.†

Another objection refers to the propriety of using an example of continuous girders to demonstrate the shortcomings of the theory, and it was remarked that the advantages of the theory would be more apparent when a greater number of spans were computed, and a greater dead-load taken. I have therefore, treated three continuous trusses of a total length of 600 feet, consisting of two outer spans, each 183 feet 4 inches, and one middle span 233 feet 4 inches long. The short spans each have 11, and the long span 14 panels, 16 feet 8 inches long. For a strain sheet with curves of moments and shearing forces, see the Plate. The calculation was made on precisely the same basis as in my paper, only the concentrated excess load of the locomotive was not considered; which, however, *was* done for two continuous spans of 200 feet each and for the single span. The figures for the three continuous spans comparatively therefore are too favorable.

By comparing the Plate here presented with that of the paper under discussion, it will be noticed that the average chord section remains precisely the same, so that in the chords, no material is gained over the two continuous spans. The average diagonal sections are larger for three spans of 600 feet, total length, whilst the post sections practically remain the same.

Now the proper application even of the usual but imperfect theory of continuity to continuous trusses, such as considered here, proves conclusively that not even the theoretical quantities of material are less than those for single spans. These theoretical quantities are expressed by the sums of the products of the length of each member into its maximum strain (respectively the sum of positive, plus negative strains).

The webs of continuous girders are still remarkably heavier than those of single spans, indicating that the heights should be taken

* For historical information regarding the theory of elasticity, reference is made to Whewell's History of the Inductive Sciences, Vol. II, pages 72, 259, 264, 442, 443, &c.

† Like many other inventors and discoverers, Euler and Bertot have not received credit, simply because writers too often accept the quotations of previous authors.

somewhat less, and it must not be forgotten that the weights of single spans, if arranged with inclined end posts, could further have been reduced.

We get the following pounds feet per lineal foot of bridge :

		Pounds Feet.		Theoretical Weight per Foot of Trusses.	Comparison.
		Separate.	Total.		
Single spans, 27 feet deep..........	Chords.	837 670	1 556 340	519	100.
Locomotive excess considered.....	Webs.	718 670			
Two continuous spans, 25 feet deep	Chords.	790 500	1 563 330	521	100.4
Locomotive excess considered.....	Webs.	772 830			
Three continuous spans, 25 ft. deep	Chords.	819 000	1 708 300	570	109.7
Locomotive excess *not* considered.	Webs.	889 300			

In case we calculate the single spans of 200 feet for double their dead loads, or 2 400 pounds per foot, instead of 1 200, and if we likewise calculate under this supposition, the three continuous spans, we get these figures :

		Pounds Feet.		Theoretical Weight per Foot of Trusses.	Comparison.
		Separate.	Total.		
Single spans, 27 feet deep..........	Chords.	1 130 000	2 045 000	682	101.5
Locomotive considered............	Webs.	915 000			
3 continuous spans, 25 feet deep...	Chords.	958 400	2 016 600	672	100.
Locomotive excess not considered.	Webs.	1 058 2 0			

The saving in trusses of continuous spans is theoretically less than 1½ per cent. Examination of these figures proves that the continuous spans are designed too deep and the single spans too shallow, and since practically the web of our single span increases but very little for one foot of increase of height, a single span of 28 feet depth would have proved as light, even theoretically, as our continuous spans under their most favorable height.

For small spans, continuity is admitted to be out of question. For railroad bridges up to 400 feet (dead not much heavier than live load), the best examples of European continuous bridges do not show less but rather greater weights than the best American single spans. For city

bridges, with heavy dead loads, greater strains can be admitted in case of single than continuous spans.

But there will be few instances in truss bridge building, under which even theoretically, the principle of continuity would seem to lead to economy. If for very large spans with limited depths, this principle is applied, it will be imperative to use hinges, and thus remove all doubtful theory.

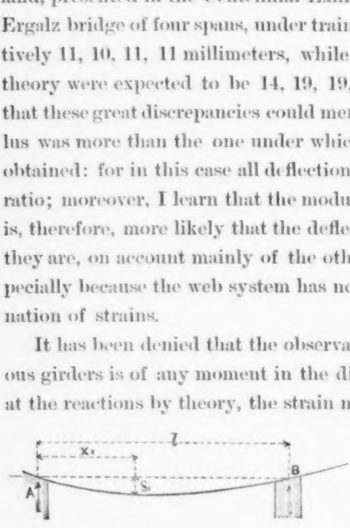
Having done my part in the discussion of the subject of continuity, it remains for those who claim any saving under the application of this theory to skeleton trusses, to point out an existing structure which can compare favorably with properly designed single spans. Will these advocates, furnish plans and calculations of their improved continuous trusses, which being correctly designed, prove to be lighter than properly designed single spans? Until this is done, I hold that hardly a theory in engineering has been presented more superficially and been worse applied, than this of continuous beams, as transferred to skeleton structures. The oversight, mainly consisted in applying the theory to skeleton structures, at all. With homogeneous beams, the formulæ are nearly in concert, and the consideration of web in this instance is quite immaterial, because the minimum webs of these homogeneous beams are already heavier than needed, and, therefore, any saving in the chords is so much gain. Lowering the middle bearings of such beams of equal section, actually leads to increased resistance, though, of course, this operation may not be reliable. But a truss bridge is not a single homogeneous beam; on the contrary, it is composed of many parts of differing sections and physical qualities, so that the application of the theory no longer is admissible. The time for continuous girders passed, when plate-webs were abandoned for bridges above 100 feet span.

I would here call attention to an error in the paper on "Revolving Draw Bridges,*" wherein it is asserted that advantage can be derived from lowering the center supports, and it is expressly claimed that a saving of from 18 to 39 per cent. of chord sections can be obtained. This mistake has crept into books, and was copied by others, because some French engineers built continuous girders with constant heights and constant cross-sections. Indeed it is possible to reduce the moments at and near the center girders, but only by increasing the moments elsewhere, so that in reality (for nobody to-day would think of not varying the chords) no saving is obtained from this construction; instead, a slight loss is incurred. On the contrary, raising a continuous bridge

* Vol. IV, page 403.

over the middle supports, or increasing the moments and the shearing forces leads to a concentration of dead load near the piers, and thereby to a slight gain of material. The writer underrates the difference between theory and practice of continuous girders, and speaks of a variation in strains of 10 to 15 per cent. only, not more than is to be expected in single spans. The reports of the North East R. R. of Switzerland, presented in the Centennial Exhibition, shows that the continuous Ergalz bridge of four spans, under trains of mean velocity, deflects respectively 11, 10, 11, 11 millimeters, while the deflections as determined by theory were expected to be 14, 19, 19, 14 millimeters. It is impossible that these great discrepancies could merely arise because the actual modulus was more than the one under which the theoretical deflections were obtained: for in this case all deflections would have been less in the same ratio; moreover, I learn that the moduli had been experimented upon. It is, therefore, more likely that the deflections appear modified, in the way they are, on account mainly of the other imperfections of the theory, especially because the web system has not been considered in the determination of strains.

It has been denied that the observation of the deflections of continuous girders is of any moment in the discussion, and that having arrived at the reactions by theory, the strain may be calculated without regard to discrepancies as to deflections.



When I rejoined, that the whole calculation and the graphical method of treating continuity rests on these deflections and on the angles of deflection, it was said that though these quantities were used in arriving at the reactions, yet they being cancelled in the final expressions, do not influence the final result, and, therefore, are immaterial.

To examine this question, let $A\,B$ represent an end span of a continuous bridge, and let δ_1 represent the actual deflection for the abscissis x_1, A may denote the calculated reaction and δ the calculated deflection. We have, according to theory, for full load, (load per foot $= p$):

$$E\,I\,\frac{d_1\,x}{d.\,y} = A\,\frac{x^2}{2} - p\,\frac{x^3}{6} + \text{constant.}$$

The constant actually is $E\,I$ into the tangent of the angle of deflection at A, and since for $x = l$, $y = 0$ we get

$$\text{constant} = p\,\frac{l^3}{24} - \frac{A\,l^2}{6}\text{, wherefrom results}$$

$$E\,I\,y = \frac{A}{6}\,(x^3 - l^2\,x) - \frac{p}{24}\,(x^4 - l^3\,x)$$

For $x = x_1$ we get the equations :

$$E I \delta_1 = \frac{A_1}{6} (x_1^3 - l^2 x_1) - \frac{p}{24} (x_1^4 - l^3 x_1) \text{ for the reality, and}$$

$$E I \delta = \frac{A}{6} (x_1^3 - l^2 x_1) - \frac{p}{24} (x_1^4 - l^3 x_1) \text{ for the calculation.}$$

Since δ differs from δ_1 also A differs from A_1; (always according to the common theory, supposing the modulus to be a constant value). By reduction we arrive at :

$$\frac{\delta_1 - \delta}{A_1 - A} = \frac{x_1^3 - l^2 x_1}{6 E I} ;$$

so that the difference between observed and calculated deflections is proportional to the difference between actual and theoretical reactions.

This investigation, as to the relation of actual deflections to actual reactions and actual strains may be extended to the other spans of continuous girders. Hence, if the calculated and actual deflections do not agree also, the theoretical and actual reactions cannot agree, and the strains consequently are not properly calculated. Differing from single spans, the deflections of continuous girders form the teststone of the correctness of calculation of strains.

Before concluding this discussion, I have to mention that it was explained* how utterly impossible it is to divide the reactions p_1 p_2, &c. of continuous girders into parts which must pass through each separate system of diagonals and posts, whilst for single spans we may follow the ways which the reactions take, because in the latter instance the distribution is, by the law of the lever, made positive.

It is unnecessary to further explain how, with continuous girders, the distribution of the reactions over the systems of diagonals depends on accidental circumstances of manufacture. Whilst I regret that this negative property of continuous girders is not fully appreciated by some, I am confident that practical men, having considered the reasons given, will comprehend the importance of this objection to manifold systems of diagonals of continuous girders.

ON THE NEW PORTAGE BRIDGE.†

Mr. Charles Macdonald.—The general arrangement of piers and intermediate spans on the Portage viaduct appears to be judicious, in view of the fact that the masonry of the old bridge was assumed to be suitable for the new. The details at the joints of the posts are quite novel and possess some merit ; scarcely sufficient, however, to warrant

* Page 162. † Referring to—CXVI, The New Portage Bridge, by G. S. Morison; page 7.

their general introduction. It does not seem to be in accordance with good practice to connect the bottoms of posts forming large iron piers, by horizontal struts. The effect of temperature upon such members, when of considerable length, must vitiate their efficacy as compression members, unless the pedestals are provided with friction rollers—and this would be a questionable expedient. I would prefer to secure the pedestals to the masonry by suitable anchor bolts, relying upon them to resist any tendency to motion by reason of the strain upon the diagonal bracing. This, of course, involves good masonry, but it is beginning to be understood that where the expense of an iron bridge is incurred, the character of the masonry should be in keeping with the superstructure.*

In the solution of the problem which was here presented to the engineer, it does not appear that sufficient importance was attached to the element of time as controlling the plan upon which the structure was to be built. The Erie Railway Co., by the loss of the old bridge, was suddenly deprived of its main line of communication with the West. It is true that a limited amount of business could be passed over the Rochester Branch, but at great inconvenience and loss of time. It, therefore, became a matter of the first importance to so arrange the design of the new bridge, that the least possible delay would result in its execution. This policy would undoubtedly lead to the use of sections and shapes of iron which could be most readily combined into the required members, and due regard would be had to the facilities possessed by the different manufacturing establishments for furnishing such material. In the pre-

* Mr. Macdonald (at a meeting of the Society, March 7th last, referring to the "New Portage Bridge") said:—The pressure upon some of the pedestal stones of this bridge, notwithstanding that it is much less than is found under the bearings of many truss bridges, has demonstrated the importance of well-bonded masonry to resist the vibrations of iron structures. Shortly after the bridge was opened for traffic, slight vertical cracks showed themselves in several places on the faces of the piers, directly under the pedestals, in some cases extending downward several feet through face stones which were laid in regular courses and had every appearance of being well bedded. These evidences of weakness, although not calculated to produce alarm for the stability of the piers, seem to be the result of concentration of load upon a few points of support, the necessary consequence of a substitution of an iron, for the broader based timber structure.

Upon the removal of one of the intermediate piers not required for the new bridge, the interior masonry was proved to be of a quality nothing better than very common rubble laid up in mortar, which scarcely deserved the name; while the face stones were *bare faced* indeed, to conceal such evidences of imperfect workmanship; yet this masonry gave no indication of its true character under the weight of the old structure, which must have brought a load upon each pier fully four times that which it now has to sustain.

It is currently reported that much of the old masonry in the country, built for wooden bridges, is of the quality above described; if this is the fact, it would be well not to trust too much to outward appearances in determining the safe limit of pressure to be applied to pedestal stones for iron bridges intended to replace wooden ones.

sent instance, the parties selected to execute the work were not familiar with the details of a trestle viaduct of such magnitude. A large portion of the material was procured from rolling mills several hundred miles distant. A premium was thus put upon delay at the beginning of the work, and a positive certainty of further loss of time insured by the adoption of a pin connection superstructure, whereby temporary trusses had to be provided in the erection.

The contractors deserve great credit for the energy displayed in completing the work in the time they did, but the Erie Railway Co. should have had the bridge completed in 45 instead of 90 days. It is believed this could have been done if the piers had been constructed of such well-known sections, as the Phœnix column—for instance—and the superstructure made in the form of rivetted lattice girders and rolled out over the piers from either side of the ravine.

In thus advocating the substitution of rivetted girders for the pin connection system so well known in this country, I do not wish to be understood as endorsing the rivetted system under all circumstances. Having had a much more extended experience with pin connections, preference has been in favor of that system; and later investigations have induced me to modify certain impressions and to accord to both systems a more nearly equal share of merit when constructed with a proper regard to correct practice.

Local considerations, such as the facilities for obtaining the required sizes of iron, ease of erection, and rigidity under a moving load, should be carefully considered in each particular case before deciding which system to adopt, and if such investigation is made without that prejudice which is too often induced by finely drawn theoretical considerations, it is believed that the decision will be quite as often in favor of the rivetted as the pin connections. Mere theorists, who have been led to favor either system, must learn to appreciate the value of experimental evidence if they would impress their views upon an interested public.

Mr. George S. Morison.—The old Portage bridge was burned May 6th, 1875. It was necessary to have the new structure completed as early as possible. To do this much of the old masonry must be used, even though somewhat injured by fire and of inferior condition. The repairs after the completion of the iron work, and the dimensions of the new iron viaduct, were adapted to masonry, which was built to carry a structure of widely different character.

Building under these conditions, the only change which I should wish to make in the arrangement of spans would be to substitute another span of 100 feet for the two 50 feet spans immediately east of tower C;* but if building an entirely new structure, in which no dimensions were fixed by old masonry, I would lengthen the trusses of the long spans and shorten the horizontal length of the towers. The present towers are 20 feet wide and 50 feet long on top, and 70 feet wide and 50 feet long at the base; a tower which should measure 20 by 10 feet on the top and 70 by 35 feet at the base, the longitudinal batter being one-half the transverse batter, would contain less material, have greater stability, be more easily erected, and the architectural effect would be much better.

As regards the details of the towers, there are few changes which I would make; the cross section of the post might be somewhat improved by increasing the relative width of the outside plate, but the connections between the posts and the struts and diagonal rods proved very convenient in erection, and is that which I should wish to adopt in another structure.

So far as I know, all iron trestles or viaducts of similar design, erected prior to the new Portage bridge, have had the feet of the posts in the towers connected by horizontal struts, both longitudinally and transversely. In the Portage bridge, the longitudinal strut is dispensed with, the transverse strut is retained and one side of every tower is placed on rollers. Mr. Macdonald has since erected viaducts on the Lake Ontario R. R., in which the pedestals are anchored to the masonry and the use of horizontal struts is dispensed with in both directions. I accept his design as an improvement, but the great width of the Portage towers (70 feet) would have rendered it inapplicable, unless a form of tower had been used similar to that sketched in the paper under discussion.†

After the bridge was opened for traffic, all the old piers were repaired; the upper surface was covered with a layer of beton coignet, the lower portion of each pier was inclosed in a crib of square timber and the space inside of this crib was filled with beton well rammed‡. The beton was forced into cavities which had been formed in the lower corners of the foundation rock, and the results have been all that could be desired.

* See Plates, page 8. † Page 7. ‡ This work was done by the N. Y. & L. I. Coignet Stone Co., under the direction of Dr. J. C. Goodridge, Jr.

The slight cracks which Mr. Macdonald refers to,* all occurred before these repairs were made, and as soon as they had been completed all settlement or cracking ceased. The results of this system of repairs, which has also been made use of, to preserve a large arch culvert of disintegrating stone in the same neighborhood, have been such that I can fully recommend it for all places where decaying masonry requires protection and it is important to avoid the expense and inconvenience of rebuilding it.

As to the method in which the contract for the new Portage bridge was given, it certainly was not that which would have ensured completion in the least possible time. The bridge was burned May 6th and opened on July 31st, 86 days after its destruction.† The true method of letting the contract would have been to give the work to three builders, all work to be done by the pound; the towers east of the main channel should have been given to one party, those west of that channel to a second, and the superstructure to a third. Had this been done and no delays been occasioned by financial troubles, the bridge would have been opened for traffic in 45 days.

The superstructure could have been built in the form of a continuous girder and rolled forward as the work progressed, but I believe the system of erection which was adopted, to be a better one. After the continuous superstructure had been brought into position, it would have been necessary to cut the spans apart, as the rise and fall of the bridge seats on the tall towers, due to changes of temperature, amounting to over 2 inches, would have been enough to throw more than double the calculated weight on a single support, and the weight of the continuous superstructure would necessarily be excessive. Ten hours' time sufficed to put together one of the hundred feet spans, when the scaffolding was ready, and if three sets of false trusses had been provided instead of two, the entire superstructure would have been ready for the passage of trains in ten days after the completion of the land tower.

* Page 236.

† The contractors to whom it was awarded had just completed for the Erie Railway, a double track in bridge of four spans, and a total length of 640 feet, weighing about 1 190 000 pounds, in just 40 days from the time the old one had been destroyed; they were fully competent to complete the Portage bridge in 60 days, though they could not have done much better. The radical mistake was made however, of ordering about three quarters of the iron from a mill in Pittsburgh, which wholly failed to furnish it at the time agreed; and 20 days after the burning of the bridge, the Erie Railway passed into the hands of a receiver, and fears of difficulty in obtaining prompt payments undoubtedly led to further delays.

ON THE ERECTION OF THE VERRUGAS BRIDGE.*

Mr. Virgil G. Bogue.—I will give an account of the railway on the line of which the Verrugas viaduct is situated, and of the peculiar state of affairs and surroundings, the difficulties other than mechanical, attending the work of erection.

The Callao, Lima & Oroya Railway has for its objects: *first*, to furnish better means of communication between the capital and certain valleys on the eastern slope of the Andes; *second*, to open up various mineral districts reputed to be rich in silver and copper ores, including the important district and city of Cerrode Pasco; and *third*, to connect as the first section of a projected railway, the Pacific with navigable water on a tributary of the Amazon.

In January, 1870, the Peruvian government contracted with Henry Meiggs for the construction of this road, and from that time the work went on with great vigor (8 000 men being sometimes employed), until the middle of August last, when operations were suspended from lack of funds. The length of the road when completed will be about 136 miles; 83½ miles of track is laid, and much of the heavy work on the remaining portion is done, so that in a comparatively short time the last spike would have been driven. The gauge is 4 feet 8½ inches.

The line commences at Callao, passes through the city of Lima and pursues the valley of the Rimac river, with a general direction north of east, 104¼ miles to the summit tunnel, where the grade attains an elevation of 15 645 feet, thence descending, the village of Oroya is reached at an elevation of 12 178 feet. From Callao to San Bartolome, distant 46¾ miles, the average grade is about 105 feet to the mile, but from there to the summit, the gradients are with few exceptions, 3 and 4 to 100. Here, too, the character of the valley changes, the mountains closing in with precipitous sides, leaving only room for the narrow river to find its way at their feet. The sharpest curve allowed, has a radius of 120 meters, and the grade in such cases cannot exceed 3 in 100.

At San Bartolome, begin the real difficulties of the enterprise, and from this place development follows development, one apparently being made that the line might arrive at a point a few miles beyond, where opportunity for another was presented, until the summit of the Andes is reached. The road throughout is one of great interest to the engineer, for its bridges and viaducts, its tunnels, enormous cuttings and fillings

* Referring to—CXIX, Erection of the Verrugas Bridge; L. L. Buck, page 103.

and its location which was the result of long and careful study. It was found necessary to develop the line at San Bartolome, where a secondary valley empties its waters into the Rimac, forming a conformation of the surface convenient for the purpose, and after making several detours in this vicinity, the road reappears upon the mountain side, several hundred feet above the river. Pursuing its course thus, it shortly arrives at the "*Quebrada*" Verrugas, which is simply a deep chasm where the mountain seems cleft in twain. Its appearance from below, with a background of lofty sierra is awe-inspiring and appalling.*

Prior to the initiation of work at San Bartolome, the difficulties encountered had been overcome with ease, and the health of the men employed was comparatively good, but shortly after, fevers, and the peculiar disease "Verrugas" (from which the canon takes its name) incident to the locality, began to appear ; from that time until the viaduct and all this section was completed, almost a plague raged among all classes. It was found that this disease, and in fact ill health in general, did not prevail in the valley below an elevation of 3 000, or above that of 6 600 feet; but between these limits it was only necessary, in some cases, that a person should remain twenty-four hours to become thoroughly impregnated with the seeds of disease. Those foreign to the country were especially liable to attacks.

Extensive quarters and hospital accommodations were provided for the men at great expense, and by these means and the payment of large wages, the supply of labor was kept up, as nearly as possible. But there was naturally a lack of skilled labor in a country where such great works had been previously unknown, and the constant depletion with resulting necessary changes going on, produced much demoralization. All materials were brought on mules from the end of the track some miles below.

* If Jules Vernes' balloon could have visited this locality, its occupants would have first remarked the narrow valley of the Rimac, shut closely in by lofty mountains, down whose sides ran deep ravines and jagged ribs of rock, all parched and barren. Drawing nearer, they would have perceived the works upon the line with thousands of men throwing down material and large masses of rock which broke up and scattered in all directions or fell with a thud and a splash into the river, while a thick cloud of choking dust covered all below. Still nearing, they perhaps could have discerned the camps and hospitals, with flags flying and queer groups of people strangely dressed in many colors. They would have heard that subdued hum which always prevails on a great work, the reports of the blasts, and the roaring of the Rimac as it plunged once, that it might more quickly plunge again. Meanwhile the sun shedding down upon all the scene its tropical rays, at last the cañon Verrugas would have burst upon them, and then verily, thinking they had arrived at the gate of the Inferno, they would have quickly sped away to more favorable parts.

Such then was the condition of affairs when it became necessary to erect the Verrugas viaduct. It will, therefore, be readily understood, that aside from the motives of economy, any method of erection which alleviated the other necessities of the case, was a great desideratum and saving of anxiety, which would set the work forward at a bound.

Mr. L. Lefferts Buck.—My account of the "Erection of Verrugas Bridge" was written in answer to questions asked at the Seventh Annual Convention, and intended only as a simple statement of facts. I have thought it probable that other questions might be asked as to the locality of the structure, etc. Nearly every mile of the railroad line from San Bartolome to the summit (a distance of about 59 miles) would furnish an interesting topic for discussion. The paper may have led some to think that we were trying to make extraordinary progress in the erection of the bridge in question. The facts were as follows: the locality being extremely unhealthy, it was desirable to get through with, and out of it entirely as soon as possible. But because of this, it was impossible to drive the laboring force, as is usual where great haste is required. It was necessary to have substantial work, and to secure this, everything was done by day's work.

Aside from the unhealthiness of the place, the only occasion for haste was to save transportation, on mules' backs, of materials to be used on the work beyond the bridge.

The station of San Bartolome is 3 miles from Verrugas towards Lima. From San Bartolome to Surco, 7 miles, the line lies along a steep mountain side, so that nothing could be gained by railroad transportation beyond San Bartolome, till the track should be laid to Surco. But between these two points are Verrugas and Cuesta Blanca. Through the latter (an immense ledge of rock) a tunnel was being driven. Now, until this was finished, there was no use for the bridge. But for every day that the bridge delayed the track after the completion of said tunnel, there would be involved an expense of several hundred dollars, due to the 7 miles of "mule packing." The tunnel, however, was the longest job, and although that work was driven night and day, we felt that we had time to work cautiously.

The first thing required, after starting on the excavations for foundations of piers and abutments, was to arrange for some security against rolling stones. The sides of the ravine being very irregular and steep—extending in altitude over a hundred feet above the road-bed, and

covered in many places with stones and rocks, liable to get loose and roll down—it became something of a problem as to the best means of protecting the work. To detach them and thus loosen the surface would make the matter worse. To dress the sides down to a safe slope, would require the removal of a quantity of material not to be thought of. Hence it was decided to build such walls of stone, backed with loose earth as a cushion, that a stone rolling down should strike nearly in the direction of the wall and be turned away from the iron piers.

In the bottom of the ravine where the highest pier stands, there was a large quantity of loose material that had been thrown down in making the deep cuts for road-bed at each end of the bridge, several months previous to beginning the bridge work. Too much time would be required for removing it all at once. Hence as little of it was removed as would enable us to prepare the foundations for the pier and at the same time render the iron work safe from any of the remainder falling in upon it.

The use of the cables in erecting the other piers, permitted of this work going on without hindrance. The difficulty of resting any false work on this material, for the iron spans on either side of the middle pier, was one cause of turning my attention to finding some means of avoiding it. It occurred to me that a temporary span would do it and at the same time be a saving in other ways. At first, the natives working below would cast an occasional suspicious glance at the heavy iron columns swinging in the air one or two hundred feet above their heads, but they soon ceased to notice it.

The bridge was ready to pass trains before the tunnel was through. I visited the structure last November. The track was as straight as when laid, although not a spike had been disturbed. The bridge being on a 3 per cent. grade, it was feared by some that the spans would move or creep on the bridge seats, but no such movement had taken place. There is scarcely any sway or side motion to the structure when passing a train; none in fact, that would be perceptible to a person not trying to detect it.

CXXV.

ON THE FAILURE OF THE WORCESTER DAM.

A Report by Theodore G. Ellis, C. E., David M. Greene, C. E., and William W. Wilson, C. E., Members of the Society.

Presented June 15th, 1876.

To the American Society of Civil Engineers:

Your Committee,* to whom was intrusted the matter of an examination and report upon the causes of the failure of the Lynde Brook Dam, at Worcester, Massachusetts, begs leave to report:

That they visited the scene of the late disaster on May 16th, about seven weeks after the failure of the dam, and made a thorough examination of the evidences yet remaining of the causes of the original leakage and subsequent giving way of the structure; together with what could be observed of the materials used and the general construction of the work. Before visiting the locality, it was supposed that it was rather a late day to examine evidences of the cause of the failure. But the examination made at the above named date shows that the removal of the debris and the excavations that have been made in the bed of the stream, exhibits some points of interest that could not otherwise have been observed. Numbers of excellent photographs were taken soon after the accident,† and part of your committee had visited the dam immediately after its destruction.

On arrival at Worcester, your committee visited Mr. Blake, the city engineer, with a view to examining the plans upon which the dam was constructed; but in consequence of instructions from higher authority, he did not feel permitted to show them. Your committee therefore was obliged to procure the information desired, from other sources.

The Lynde Brook reservoir lies in the town of Leicester, about 4 miles west of the city of Worcester, and 1½ miles north of the track of the Boston & Albany R. R. The main part of the dam runs nearly east and west, across the bed of the stream, which flows to the south. This part of the dam

* By vote of the Society, canvassed April 5th, 1876, appointed "to examine and report upon the recent failure of the dam at Worcester, Mass." † Some of which are in the Society's library.

is about 600 or 700 feet long. The east end curves to the north and extends about 1 000 feet across a low divide, to prevent the water from wasting in that direction. This dam was built in 1870 and 1871. The pond was of about 132 acres in area, and contained when full, 663 330 000 gallons.

The failure occurred in the main body of the dam, in the bed of the brook, in the line of the gate-houses and where the supply pipes passed through the embankment.

This main dam consisted of an earth embankment, composed of an excellent material, brought up in layers of about 6 inches, and thoroughly worked together. In the centre was a wall, built chiefly of cobble stones laid in hydraulic cement. It was intended to be 3 feet thick, but the part remaining, measures about 33 inches. This wall was constructed by building up the outside a short distance, and filling the interior with concrete. The cement used was of good quality, and the work was tolerably well done, although such a wall is far from being water-tight. In the bed of the brook, through the dam, was an arched culvert for containing the supply-pipes, 8 feet wide and 8 feet high in the clear. The side walls were 5 feet thick, and extended 2 feet below the top of the paving. The arch was 18 inches thick, and was covered on top with a layer of cement and brick. The paving consisted of 8 inch cubes of granite, under which were flag-stones lying upon about 10 inches of broken stone. The side walls, arch and granite blocks were all laid in hydraulic cement of good quality ; the side walls being built double and filled in with concrete.

At the upper end of this culvert was the main gate-house, a very substantial structure of stone, lined with brick, extending from the foundation of the dam to the top, with a chamber above that level which was roofed and furnished with doors and windows. At the lower end of the arched culvert there was another gate-house, of small size, one story high, provided with a door and windows, containing the waste-gates. From this gate-house a round stone culvert reached some distance into the stream below, to carry off the waste water. The floor of the gate-house was paved with 8-inch cubes like the arch, but resting on the natural earth. These gate-houses were situated somewhat inside of the foot of the slopes, and wing-walls were built of dry rubble to hold the foot of the slope.

The top of the embankment was about 50 feet wide, with a slope of 2 to 1 on the upper side, and a slope of 2½ to 1 on the lower side. Its height at the gate-house was 41 feet, being 4 feet above the sill of the

waste-way. The arched culvert pierced the central wall which was built up to it on all sides. Between the upper gate-house and the culvert was a strong bulkhead of masonry, about 10 feet thick, laid in cement, through which passed the two 24-inch supply-pipes. This bulkhead does not seem to have extended in any direction beyond the gate-house.

At the west end of the dam was a waste-way, 22 feet wide, which extended along the side hill to some distance below the end of the lower slope. This waste-way had one side wall on the down hill side laid in cement, and a concave bottom of rough stone paving, laid dry, about 2 feet thick. Over the central wall, a cut granite sill was laid, close up to which, came the dry paving on the upper side. This paving was a continuation of the rough stone facing, of about 18 inches thick, covering the upper slope of the dam. Before the construction of the dam which failed, there was a dam at this place of less height, which was covered up by the new dam when built. This old dam had a central wall of masonry, similar to that above described, except that it was thinner and was built in a single wall which now shows in the sides of the excavation. The middle portion of the old dam was removed in digging the foundations for the masonry of the new dam. The part now washed out shows that the sod was not removed from under the old dam, but was removed in the new construction. Particular inquiries were made regarding the foundations of the masonry of the arched culvert and gate-house where the failure took place, and it was ascertained that after the arch was built and it was desired to put in the paving, it was found that it could not be done as intended, on account of the softness of the bottom. At the upper end of the arch, the ground could be shaken by stepping upon it, and it was wet and springy.

The way in which the paving was laid, was by pressing into the ground the rough stones before mentioned and then covering them with the flag-stones, upon which the granite blocks were laid in cement. A like difficulty seems to have been experienced with the foundation of the upper gate-house. Here it was necessary to press into the moist and springy earth, about 3 feet in thickness of broken stone, before the masonry in cement could be commenced. This gate-house does not appear to have settled previous to the giving away of the dam.

It is stated in the newspaper accounts of the disaster, that there has been a leakage into the culvert ever since the dam was completed. The commencement of the failure was about two years ago, when water was first observed leaking in considerable quantities into the

pipe culvert, about 20 feet above the central wall, and passing out through the arch and lower gate-house. In April, 1875, the leakage was at the rate of 24 221 gallons in twenty-four hours, with the water 6 feet below the waste-way. In July, under a similar head, it had increased to 39 810 gallons per day. On September 7th, 1875, the city engineer called attention to the increased leakage, and recommended "that the pipe arch be thoroughly cleaned of all deposit, which will enable careful inspection to determine, if the water is discolored on entering the archway, which would indicate an increase of wash under the foundation." Under date of December 1st, 1875, he reported that the clearing out of the deposit had been done, and that a weir had been placed in the pipe culvert itself, where only the water entering through the leak could be measured. Previous to this time the only means of measuring the leakage had been by a weir below the outlet of the waste culvert, which was defective and unreliable at times from the effect of surface water. On this date, the engineer reports, "the leakage at present is 48 448 gallons per day, showing an increase of about 24 000 gallons since April 1st. It being important to locate the leakage, two weirs were set in the arch, one a short distance from the upper end, and the other near the lower end. It is a matter of congratulation that the investigations prove the leakage is confined entirely within a short distance of the upper end." During the year ending December 1st, 1875, the reservoir was not at any time full, the height of water being from 3½ to 19½ feet below the level of the waste-way.

In the above-named state of affairs, in the opinion of your committee, it was only a question of time when the dam should be destroyed, and it seems to be somewhat remarkable that this should not have been appreciated by those having charge of it, and immediate steps taken to repair the damage and stop the leak. Nothing, however, appears to have been done, and when the reservoir filled up in the spring of 1876, the leak largely increased. Two days before the final giving way of the embankment, large quantities of muddy water were observed to be flowing from the waste culvert below the lower gate-house. An examination was made, and the water was found to be entering at or near the place of the old leak. The flow rapidly increased, until just before the final catastrophe, the water rose in the lower gate-house so as to flow from the door and windows, being more than the waste culvert could discharge.

The first caving in of the embankment took place on the west side of the upper gate-house, after which it went rapidly, the central wall

checked the rush of water for an instant, and then the whole gave way before the vast volume pouring from the reservoir above. The upper gate-house fell over to the southwest, against the sloping side of the chasm and mostly broke up, though some portions of the masonry remained together.

In the opinion of your committee there is no doubt as to what was the immediate cause of the failure of this dam. There was evidently a stratum of porous material lying under the upper gate-house and the upper end of the pipe vault, partaking of the nature of a quicksand, which should have been removed, and greater precautions taken to prevent the access of water from the reservoir, than appears to have been the case in the foundations before described. The water found its way through the porous bed on which the gate-house and arch foundations rested, and leaked through the interstices of the masonry of the vault until it wore a passage large enough to carry the earth with it, after which the destruction proceeded with great rapidity.

The details of the catastrophe derived, chiefly from the accounts in the *Worcester Spy*, are as follows :*

During the day preceding the final failure, attempts to stop the leak had been made in various ways without much effect. Some time before daylight on the morning of the day the dam gave way, Thursday, March 30th, a section of the dam between the upper gate-house and the waste-way or "roll-way" as it appears to have been generally called, about 20 feet wide and 5 feet thick, caved in. The water followed with a rush and a roar, and above all was heard the grinding of the stones of which the upper face of the dam was composed, as they rolled into the breech. The water then poured in torrents through the doors and windows of the lower gate-house, and the dam was momentarily expected to break. Work was however continued, bags of hay loaded with stones were thrown into the water at the break, and after an hour the flow perceptibly decreased, and the rumbling of the stones as they ground over each other was no longer heard.

With the dawn of day the exertions of the workmen were renewed. The barns in the neighborhood were opened, and the hay loaded in carts and drawn upon the dam, where hundreds of men were only waiting for an opportunity to assist in averting the catastrophe. Axes were brought, and the large pine trees in the grove below the dam were cut down and hauled as near the break as it was deemed safe for horses to go, when they were taken up by the men and thrown into the gap.

* They are added as a matter of interest.

At this time, the portion of the dam below the lower gate-house was badly gullied and the stream through the door was falling into a hole about 20 feet deep. An immense quantity of water was flowing through the break, yet the stream above was discharging almost an equal quantity into the reservoir above, so that little headway was made in drawing off the water. During the day, canals were being cut through the eastern embankment of the dam, so as to allow the water to find its way down through a meadow to Parsons brook. The ground was frozen and this work proceeded very slowly. The cement "spiling wall" was cut through with difficulty, and it was not until 4 o'clock that a passage was made. At this time the leak through the dam had diminished, and it was hoped that by drawing the water off, the danger would be partially averted.

But this hope was without foundation. At ten minutes of six, the water was seen bubbling up through the earth of the dam back of the waste gate-house. The stream when first noticed, was not larger than a man's finger. In less than a half a minute, it was as large as a man's wrist. At the end of a minute, it was as large as a man's leg. In another minute it was as large as a barrel, and was 3 or 4 feet high. Then stones and earth were thrown up in the stream and gradually the earth began to cave in. Foot after foot of the solid embankment, now thoroughly honeycombed by the action of the water, gave way. Then sections varying in width from 2 to 5 feet, began to drop off, until the embankment was gone back to the face of the dam. The cement spiling wall held for a moment after all the earth had been washed away. Then a hole appeared in the centre of the wall, gradually its size increased, until with one grand crash the spiling wall crumbled, letting loose the 760 000 000 gallons of water stored behind.

The water rushed down the ravine in a solid mass, 20 feet high. First in the line of the flood was the stone waste-gate house. When the flood struck this, it tottered, then the key-stone of the arch dropped out, a corner of the building next gave way, followed by the wooden roof, which was swept onward until drawn into a whirlpool, when it was crushed to matchwood and thrown into the air. The gate-house was tipped over bodily, and not even a stone of it has since been seen. When the dam first broke, the gap was about 20 feet in width. This increased rapidly after the water had once gained a passage. and continued to increase, until the width was about 80 feet. The ravine below being narrow, held the water back, and it continued to run for about three hours before the reservoir was exhausted. Another account says :

At twenty minutes of six, the earth at the surface in the rear of the lower gate-house suddenly became very muddy, and the water bubbled through in another instant. The scene for the next five minutes was terrible yet grand. The water continued to ooze through the earth, and in about two minutes, a small stream was running. Two minutes later, the stream had increased, and at the end of seven minutes the water, which had worn a deep gulley, began to work backwards toward the break in the face of the dam.

The trees, stones, bales of hay, and timbers, thrown into the breech, were thrown through in a violent manner, and every moment the break was enlarging, until only the spiling wall remained. This gave way in another moment, and the space at the top was about 15 feet wide, through which the torrent was pouring and continually enlarging, until the rush of water was over, and the waters of the reservoir had been sent down upon the valley below.

During the first hour, the water was drawn down about 10 feet. The upper gate-house stood until nearly the close of the first hour, when the masonry gave way bodily. The foundations fell across the stream. The upper portion was wrecked in an instant, and the granite blocks of which it was built, added to the boulders passing down the stream, increased the rumble and roar, which was heard above the noise of the rushing torrent. For three hours the flow continued, until the gap was 150 feet wide at the top and about 50 at the base. By eight o'clock the reservoir was nearly emptied. *(Signed by the Committee.)*

MR. WILLIAM J. MCALPINE.—Soon after the disaster, which is the subject of this paper, Messrs. Cunningham, Francis, Shedd, Worthen and myself, members of the Society, were appointed by the corporate authorities of the city of Worcester, a commission, to examine and report upon the causes of the failure. This commission spent days in searching for evidence, from those who built and those who maintained the dam, those who witnessed the devastation its failure caused and others whose professional and personal knowledge of the fact qualified them to speak as experts.

I cannot assent to the conclusions of the report just read. At an early day, I hope to be able to present to the Society my views of what led to this disaster, founded upon the evidence thus obtained and such observation and experience as has been possible for me to bring to bear upon the case.

AMERICAN SOCIETY OF CIVIL ENGINEERS.

INCORPORATED 1852.

TRANSACTIONS.

NOTE.—This Society is not responsible, as a body, for the facts and opinions advanced in any of its publications.

DISCUSSIONS OF SUBJECTS PRESENTED AT THE EIGHTH ANNUAL CONVENTION.*

ON GAUGING OF STREAMS.†

MR. DAVID M. GREENE.‡—In the examination of questions frequently arising among neighboring mill owners using the water-power of our country streams, it almost always happens that the engineer is called upon to form an opinion as to the adaptability of the machinery used, to the available power of the stream, and as to the "reasonable use" of such power.

It is assumed that so much machinery may be employed, at any point upon any stream, as can be effectually and fully operated by the water flowing therein at its "ordinary stage." What, then, is the ordinary stage of a stream? We answer, that it is the minimum flow occurring during from eight to nine months of the year, excluding the three or four months of low water usually occurring during the late summer, early autumn and in mid-winter. In other words, machinery properly adapted to the flow of water in the stream upon which it is located, may run without interruption, from a defective supply of water, during eight to nine months in each year.

The extent of the interruptions which are to be expected during the remaining three or four months, will, of course, depend upon the extent to which extreme low water falls below the ordinary stage, and upon the duration of the extreme low stage.

* Continued from page 244.

† Referring to—Report of Committee on uniform Gauging of Streams, in Connection with Observations of Rainfall; T. G. Ellis, Chairman; Proceedings, Vol. II, page 61.

‡ Presented June 10th, 1876.

It is assumed that such an adaptation of machinery is a fair compromise between the condition of an amount of machinery adapted to the maximum flow, which would be subject to constant interruption throughout the entire year, and that of machinery adapted to the minimum flow, which, while able to run uninterruptedly, would involve a constant and very large waste of power, except during the generally brief period of extreme low water.

The charge of "unreasonable use" is usually made when, during low water, mill owners draw more water than is naturally flowing in the stream, until their ponds are exhausted, or their heads reduced to such an extent as to necessitate the stoppage of their works, and then shut their gates for the purpose of filling their ponds; thus entirely suppressing, for a longer or shorter time, the flow of water, and of course, interfering with the operations of machinery below.

Again, the charge of "unreasonable use" is sometimes made by manufacturers whose business requires them to run their machinery night and day, and who therefore, require a constant and uniform flow of water; when their up-stream neighbors, running their machinery during the day only, draw down the water during the day, and at night shut down their gates in order to accumulate a supply of water for the following day, thus sending down an unusual and unnatural quantity by day, and almost entirely suppressing the flow at night.

It is not, however, my purpose to discuss the various and complicated questions growing out of the use of water power, but rather to indicate the means I employed in an important case, to determine the minimum flow of a stream and its relation to the use by a cotton factory, as a means of ascertaining, approximately, whether the quantity of water used by the factory was or was not greater than the ordinary flow of the stream.

In the case referred to, an improved Fourneyron turbine about 8 feet in diameter was used, under a head of 40 feet. This turbine was so carefully constructed, that the quantity of water used upon it could be determined very accurately, and the dam was tight.

The observations were made during the period of extreme low water, in summer, and in the following manner: *First,* the pond being full to the crest of the dam—no water flowing over—the factory was started up and run, to its full capacity, until the pond had been drawn down about 2½ feet. *Second,* the gate was then closed tightly, and the pond allowed to fill to the point at which it stood when the machinery was started.

Third, the times required to draw down the pond and to fill it again were carefully noted.

These operations were repeated several times—drawing the pond down various distances, ranging from 4 inches to 2½ feet. While drawing down the pond, in each case, a determinate quantity of water was used by the turbine which was equal to an indeterminate quantity stored in the pond, plus the indeterminate quantity flowing in the stream in the same time. While the pond was being raised to its former level, in each case, the above indeterminate quantity of water was being stored therein from the normal minimum flow of the stream.

In order to express the relations analytically, to eliminate the volume of water stored in and drawn from the pond during each experiment, to determine the relation between the volume flowing in the stream and the quantity used by the turbine, and to ultimately determine the minimum flow of the stream, we proceed as follows:

Let Q, equal flow of the stream, and Q', the flow through the turbine, in cubic feet per minute; t' the time required to draw down the pond, and t for pond to fill to original level, in minutes, and V, the volume stored in and drawn from pond, during experiment, exclusive of flow of stream.

Then $V = Qt$, and the quantity used by the wheel, while drawing down the pond, $V + Qt' = Qt + Qt' = Q't'$; whence

$$\frac{Q}{Q'} = \frac{t'}{t + t'}; \text{ and } Q = Q'\left(\frac{t'}{t + t'}\right)$$

Several experiments were made, in which the value of $\frac{Q}{Q'}$ ranged from 0.65 to 0.69; showing that, at extreme low water, the stream furnished about 66 per cent. of the quantity of water required to run the factory continuously, at its full capacity; or considerably more than would be required to run at full capacity during 10 hours of each day, provided the flow could be controlled during the night.

Similar experiments, made at the ordinary stage of the stream—or at any stage—when the flow is less than the quantity used upon the wheel, would furnish the relations between Q and Q' at those stages.

This method—original with the writer—is believed to be far more satisfactory and to furnish much more reliable results, in cases similar to that herein described, than the usual rude and uncertain methods in which sections and velocities of the stream are taken, or where the discharge over dams is estimated.

ON THE CROTON WATER WORKS AND SUPPLY FOR THE FUTURE.*

Mr. J. Herbert Shedd.†—The paper under discussion presents in a striking manner the immediate need of attention on the part of the city of New York, to her public water supply. Enormous interests are dependent upon the integrity of the works which furnish this supply, and the present close approach to the maximum power of delivery will not admit of much delay in making provision for an increased supply, or for a more perfect utilization of that at present furnished to the city. The remedy which can be most quickly applied, in the present condition of things, will have, on that account, great advantage over that which requires more time for its application. It may be that, for security, a second line of aqueduct, so located as to be safe from injury by a breakage of the first, will, before a long time, need to be constructed; but this will require so much time for completion, unless so hurried as to greatly increase its cost, that means for lessening the waste within the city will be imperatively needed before an additional supply can be made available in that way. And if duplicate lines are constructed across the valleys and in such other places as are most liable to failure, the lessening of waste may defer for many years the need of a complete duplicate line, thus saving large sums, in interest on construction,

Any schemes for furnishing water that is unfit for domestic supply, but suitable for certain other large uses, for the purpose of relieving the draft upon the present supply, will be accompanied with such complication and expense as to rule them out of the question except as a last resort.

It appears that the present consumption is more than 100 gallons per day for each person in the city, including visitors. This is a larger supply than can possibly be needed for any useful purpose, and it is safe to say that one-half the amount is preventable waste. To stop this waste, will be about equivalent to doubling the present capacity of the works, and I am satisfied that it can be done at moderate expense, without detriment to any interest of importance, and without annoyance to takers that will be objected to by any fair-minded person. This prevention of waste will bring advantages to the lower part of the city that cannot be accomplished by an additional conduit alone; it will increase the pressure throughout that district, thus supplying water to high portions

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* Referring—to CXX, Notes and Suggestions on the Croton Water Works and Supply for the Future; Benjamin S. Church, page 107. † Read at the meeting of the Society, April 5th, 1876.

of buildings, and furnishing an abundant supply through the distribution for fire purposes. It would have this further advantage, that the storage capacity within the city would then be sufficient to allow time for needed repairs and for strengthening the conduit on embankments, and if necessary, the connection of duplicating lines across the valley.

That half the quantity of water now supplied to New York, is uselessly wasted, is proved by comparison with the experience of many other cities. The most plausible pretense in thus allowing water to run to waste, is that it serves to keep the sewers clean; but the amount that might still be spared after half the flow had been stopped, if judiciously used in flushing the sewers, would be of more value than the continuous dripping that now amounts to about the same as ¼ inch of rainfall per day.

The city of Providence, where the daily supply is less than 22½ gallons per inhabitant, may be cited as an example of what can be accomplished in the economical use of water. It is not known that any resident of that city feels under restraint in the use of water, either for domestic, manufacturing or other purposes, except so far as to prevent needless waste. There is probably a sufficient supply at the source for 2 000 000 people, and the works are constructed on a scale sufficient to furnish about five times the quantity now delivered. It is believed that the water is liberally used for all purposes, but is not, to a large extent, wasted. This result is undoubtedly owing, in part, to the works being new and thoroughly built, so that the leakage in pipes and fixtures is small, and, in part, to the character of the fixtures employed, but it is believed to be mainly owing to the unprecedented number of meters in use.

The population of Providence in 1875 was 100 675, on an area of about 15.5 square miles. The area which is considered thoroughly piped by the water distribution is about 7 square miles, within which is an estimated population of 91 436. About 114 miles of main water pipes are laid within this territory, supplying 9 700 families and large manufacturing and other interests; 510 steam boilers are operated within the city, and 6 624 vessels arrived in the port during 1875. Up to April 1st, the total number of services opened was 5 981, and the number of meters in use was 2 426.

The population now supplied with water for domestic purposes is estimated at 48 000. The large number of meters in use enables a more accurate estimate of the quantity required for various purposes, to be made than would otherwise be possible. The use for manufacturing purposes

is seen to be very large, while the use in dwellings is about one-third the total supply.

The daily use of water, as measured and estimated, is as follows:

	Gallons.	Per Cent.
For manufacturing	984 000	44
Dwellings	720 000	32
Other purposes (including leakage)	546 000	24
Total	2 250 000	100

It appears by the United States census of 1870, that the number of persons employed in manufacturing interests in Providence is proportionally greater than in New York, while the number engaged in trade and transportation is proportionally less; neither interest varying largely from the proportion of population. As the amount of water required for manufacturing interests is greater *pro rata* than for commercial, it seems fair to assume that a direct comparison of Providence with New York would indicate a full supply of water to the latter city for all useful purposes. It appears above, that a little more than half the population within the thoroughly piped district is now supplied for domestic purposes. It is estimated that about three-fourths the manufacturing interests are supplied, and the use of water for other purposes may be assumed to be in proportion to the population supplied.

For an approximate estimate, the following quantities give a full daily supply to the entire population of New York for all useful purposes without wasting:

	Gallons.	Per Cent.
For dwellings	15 000 000	38
Manufacturing	13 120 000	33.2
Other purposes (including leakage)	11 392 000	28.8
Total	39 512 000	100

Or say a daily supply of 40 000 000 gallons, while the supply now delivered, as estimated by the Water Department, is understood to be about 100 000 000 gallons, or according to Mr. Church's estimate, 114 000 000 gallons.

It will probably be difficult, but not impracticable, to reduce the present waste, without lessening the amount fairly used, so that the daily delivery shall be 40 000 000 to 50 000 000 gallons. This very desirable and important result can only be accomplished by care and attention in various ways. The Providence system of assessing water rates on the opportunities to waste water instead of on the valuation or size of building, would be a valuable aid in this direction. A family supplied by one faucet is there charged $6 per year ; each additional faucet or fixture on the premises involves an addition to the annual rate; and for those who are willing to be careful in the use of water and wish to lessen their rates, it is provided that a meter may be put in, subject to certain conditions.*

Under this system nearly all the water meters in use in Providence are owned and maintained by the consumers, only 10 of the 2 426 meters now running being owned by the city. Of course the same saving of water may be attained by the use of meters owned by the city, if there is objection to changing the rates, so as to cause private parties to purchase them.

But many causes of waste exist that would not be reached by individual meters under the above system ; these need attention by inspectors, but the examination by these officers ought generally to be made without intruding upon private premises, to the annoyance of the occupants, except where waste has, by outside inspection, been found to exist. Such inspection is quite feasible, even in so large a city as New York, and the discovery of sources of waste should include leaks in the street mains and services as well as waste through private plumbing. It is found by measurement that a single faucet in my house will discharge more than 22 000 gallons of water per day, if left running all the time, and it is therefore evident that but a very small proportion of the faucets in New York would need to be left open to discharge all the water that is counted as waste. Leaks have sometimes been discovered in street mains on old works, that discharged a much greater amount than this faucet, and yet the water did not show itself at the surface of the street. Imperfect fixtures and fixtures of unsuitable design furnish another cause of very

* These are as follows : "when a consumer shall prefer to pay the cost of such a meter as shall be approved by the Commissioners, together with the cost of putting in and of maintenance, rather than to pay schedule rates, or for the quantity estimated, a meter will be put in ; provided, however, that in no case where a meter is used shall the annual charge be less than $10. The Commissioners reserve the right to put in a meter at the cost of the city, in any case, and charge for measured water, instead of being governed by the above schedule."

"When water passes through a meter it may be used for any and all purposes. No service pipes, however, will be allowed to be laid across a street."

serious waste of water and a system of inspection and approval of fixtures before they are allowed to be used will be an important aid in reducing waste. Plumbers should be held strictly responsible for the tightness and good character of their work, and fined or stricken from the list of approved plumbers when it is found to be below the proper standard.

The cost of reducing the waste of water in New York, if the work is managed by judicious and efficient men, who are acquainted with the subject, will be but a very small part of the value to be gained by such reduction.

MR. JAMES B. FRANCIS.*—The conduit of the Cochituate Water Works supplying the city of Boston in embankment, consists of two rings, of 8 inches of brick, depending entirely on the earth for support. The embankments were carefully formed and well consolidated for the purpose of affording sufficient support to the brick work. For many years the conduit has been strained beyond what was intended, by running the water under a head; that is, if an opening was made in the top, the water would rise in it above the top of the inside of the brick work.

It is cracked longitudinally in a great many places, which has been attributed to this excessive pressure, but I think this is not the whole cause; one reason for thinking so, is that the cracks began to form before the conduit was run under a head, and it is said, before water was admitted at all.

But there is another cause acting in the embankments, which may also have had an effect on the Croton aqueduct, I mean the frost. A systematic examination† of the interior of the conduits, November 20th, 1873, showed that it had been cracked and repaired in great part of its length, and was wider than its original dimensions both in cutting and embankment, the change being much greater in the latter, in which the widening was generally from 1 to 3 inches. The worst place reported, is at the high embankment at Newton Lower Falls, where it is from 0.3 to 0.48 feet wider than originally built. In this and many other places repairs have been made repeatedly, on each occasion undoubtedly leaving the conduit a little wider than it was before.

The effect of frost, I think, must be to relieve the sides of the conduit, of part of the pressure of the earth. In the accompanying sketch I have represented the frozen ground as being 3 feet deep, which is a common

* Read at the meeting of the Society, April 5th, 1876.

† By Mr. David W. Cunningham, first assistant Engineer of the New Supply for Boston. See City Document No. 134, City of Boston, Report of the Cochituate Water Board, January 7th, 1874, page 34; (also same—No. 103, May 1873, page 25, giving results of examination, October 12th, 1872).

depth in this vicinity, and it is frequently more. The frozen layer expands, which the shape of the bank evidently permits, tending to relieve the sides of the conduit from part of the pressure of the earth; when the earth thaws, there is no tendency to go back, and the effects from year to year must accumulate. I do not advance this theory as being proved, but as a probable explanation of part of the trouble found in such conduits.

MR. WILLIAM R. HUTTON.*—The original defect in the Croton Water Works, and which appears to be the cause of the numerous and recurring leaks described in the paper considered, was the support of the aqueduct upon a dry stone wall. This, by its character is exposed to air, moisture and pressure, is in the best possible condition to promote disintegration of its materials and "those minute but constant movements always existing in dry foundations."† I attribute the cracks in the aqueduct entirely to settlements from this cause, and I doubt whether any thickness of solid masonry or bottom would prevent them, while the same character of foundation remains. Although I do not know the fact, I suspect that in light cuttings, on good foundations, the cracks are wanting, and this would, if true, indicate that the sidewalls with earth filling are sufficiently heavy.

Upon the Baltimore Water Works, the conduit was carried over the few low places crossed by it, on walls laid in cement mortar, with side walls and spandril filling to the arch, and has given no trouble whatever. This is a most satisfactory method when the quantity required is so small as to permit the expense.

The circular conduit of the Washington Aqueduct of 9 feet interior diameter and 14 inches thickness, is carried over ravines on embankments of rammed clay, constructed with great care, spread in 3 inch layers and thoroughly rammed with 60 pound rammers. The bank having been raised to the centre line of the conduit, the lower semicircle of earth was cut out, and the masonry laid directly upon it without other foundation or extra thickening. The upper semicircle was afterwards built, and the whole backed and covered with rammed clay. Some little time after completion there was found over every embankment a longitudinal crack in the crown of the arch, its width varying generally with the height of the embankment, but greater on the newer banks, which had not been allowed time to settle before the construction of the masonry. In some cases there was a fine longitudinal crack in the

* Read at the meeting of the Society, April 19th, 1876. † Page 107.

bottom; generally, on moderately high embankments, a transverse crack across the crown at each end of the bank.

At the lower end of the conduit it is exposed to an upward pressure at the crown, due to a head of about 3 feet. Over a new embankment it cracked badly (opening probably ⅜ inch) and water leaked through the embankment. The bank was afterwards raised and widened, and I am informed* that it has given no trouble since.

The substructure of mortared masonry, built up from solid earth or rock, is, without question to be preferred, where the cost is not too great. In most cases it will not be admissible. Next, I should prefer the light masonry enveloped in water-tight material; every precaution being taken to prevent or diminish settlement.

The necessity of transverse stop-gates at the waste-gate houses, as referred to by Mr. Church, is easily understood—but the question at once suggests itself—why have not these been provided?

The direct connection of mains with conduit, as suggested, would have the desired effect in keeping full head on the mains while the reservoir was filling.

The remarks in the paper as to waste, and means of preventing it, commend themselves as judicious and sound. Under the state of affairs as here presented, it should be considered indispensable to commence at once the construction of other means of supplying New York with water, and this, whether meters are supplied or not.

Mr. William J. McAlpine.†—The Croton Aqueduct was designed and built by John B. Jervis, whose work is regarded by the profession in this and other countries, as "one of the greatest achievements of modern engineering,"‡ and has been followed even in its details of plan and execution, on all similar works in the United States.

This paper states that certain works of the original construction have failed, gives the writer's opinions of the causes of such failure, and states how they might have been avoided. In substance it is alleged, that the plan for supporting the aqueduct over depressed places was defective in dimensions and material, for the purposes for which it was designed.

The object of the following remarks by one of Mr. Jervis' pupils, is to examine whether the causes thus assigned for the failures stated, were sufficient, simply or combined, to produce them; whether the plan suggested by the writer would have prevented the failures, and whether, in

* By Mr. T. B. Samo, Engineer in charge of the work. † Read at the meeting of the Society, April 19th, 1876. ‡ Page 107.

fact, the failures are not chargeable to a misapplication of the structure to purposes for which it was not designed.

Mr. Church states* that longitudinally (in the line of the aqueduct) fractures in the upper and lower brick arches and concrete have occurred wherever the conduit rests upon the dry stone walls used for the support of the aqueduct in crossing depressions below the grade of its bottom, and attributes these fractures to the combination of the following causes :†

First. Insufficient strength in "the masonry enclosing the column of water,"—"to resist lateral hydraulic pressure of full water flow." *Second,* "The friction from minute but constant movement always existing in dry foundations." *Third,* "The settlement, in some places, of these foundations," "the tendency of which is to spread the wall." "Consequently on all of these embankments the aqueduct has split longitudinally through the top and bottom being torn asunder by the above mentioned forces."

Allusion is also made to the effect of "sudden and severe changes of weather, and to the reopening of the fissures, due to increase of lateral pressure (of the water) from the greater depth of flow in the aqueduct. The writer also adds‡ that "the outside protection walls have been raised and strengthened, and the embankments more thoroughly drained, which, to a certain degree, has prevented the further settlement of foundations and put the aqueduct in somewhat stronger condition than in former years, to sustain its increased burden." And further, "had the bottom of the aqueduct, instead of being formed of 15 inches of concrete and 4 inches of brick, been made 3 feet thick of solid masonry, lined with 4 inches of brick, and correspondingly increased thickness of sidewalls, these longitudinal ruptures would hardly have occurred, as therein would have been contained requisite strength on embankments, where alone the aqueduct has proved weak."

When the Croton Aqueduct was first brought into use, some very small transverse cracks in the conduit occurred, generally where the foundations changed abruptly from the supporting walls to solid rock. The weight of the aqueduct on embankments and of the earthwork and outer walls above the grade of its bottom, was from 30 to 40 tons per lineal foot ; that portion which the foundation walls had to carry, including the slopes of saturated earth and the conduit full of water, did not exceed $1\frac{1}{2}$ tons per square foot of its top surface. These walls were

* Page 108. † Page 107 and following. ‡ Page 109.

founded upon the rock on solid earth, and were formed of sound, well-shaped gneiss, thoroughly bonded and laid up with unusual care. The top courses were formed of the largest and best shaped stone and laid out with even additional care.*

The load which these foundation walls brought upon the earth foundations was not enough to produce any appreciable yielding after the conduit part was commenced, except perhaps in a few places, and the compression or crushing of the masonry itself could not have been increased to any considerable extent between the time that the aqueduct was complete and when it had been subsequently filled with water. The fine hairline cross cracks, which exhibited themselves occasionally for a few years, showed that there was more compression in these artificial than in the solid rock foundations.

The conduit at that time was not unfrequently filled to within 2 feet of the intrados of the upper arch, and therefore the load upon its bottom and the foundation walls was only 2 per cent. less than has, since that time, been imposed by the "full flow of water." This increase has been gradually reached after many years, and is now too inconsiderable to produce any appreciable increase of settlement; but if it did have such an effect it would have been shown as in the previous cases by transverse cracks, and only at the considerable and abrupt changes in the elevation of the supporting walls.

The fractures described by Mr. Church are all longitudinal and extend over the 125 pieces of embankment, which are from "100 to 1 400 feet in length, and from 10 to 40 feet in height."†

When the aqueduct is full of water, the load which it imposes upon the invert is but one-tenth as much as that which the side walls, top arch and the sloping prism of earth above them impose upon their bases. The inverted arch and concrete distribute each of these weights nearly equally over the top of the foundation walls, and the more equally when there is the most water in the conduit.

We must therefore conclude that the longitudinal fractures described, have not been produced by the settlement of the foundation walls, or by the crushing of the stone of which they are formed.

The fractures at the bottom are described as extending through the brick and concrete and allowing the water to escape through the dry masonry below; and in another place it is stated that the more thorough

* I speak from frequent personal observations made during the whole building of the work. † Page 109.

drainage "to a certain degree has prevented the further settlement of foundations."* These remarks are significant of another cause for the fractures than any which have been mentioned by the writer. His expression by "the friction from minute but constant movement always existing in dry foundations," † is not very explicit; but from oral statement, I understand him to mean that these foundation walls laid dry, under the circumstances of this case, are always subject to a minute but constant movement of the stones (forming the wall) upon each other, the friction of which produces a minute settlement and spreading of the wall.

As has been previously stated, 90 per cent. of the whole weight, which rests upon the foundation walls, has been a constant load from the day when the aqueduct was finished, and the added load of water has been almost a constant throughout each year, except when in the earlier use of the work, the water was once a year drawn off and re-filled, which the paper states now requires thirty hours; that is, the lessened or increased load obtained each hour was but 0.25 per cent. In latter times, whenever a "full flow of water" was drawn off and the conduit refilled, the lessened or increased load per hour was but 0.33 per cent; and these changes may have occurred several times in some years. These very small changes in the weight imposed upon the foundation walls, brought on so slowly, could not have produced any lateral movements among the stones, so as to have caused any friction, wear or settlement. This almost constant load, almost equally distributed over the top of these well binded walls, would have no tendency to spread the walls, nor would that effect be produced by the small settlement either at the bottom or in the very small amount of crushing of the stone, under a weight, already stated, at 1.5 tons per square foot at the top.

The writer expresses an opinion that the fractures are in part due to the "lateral hydraulic pressure of full water flow;" and in another place also in part due to the yielding of the side walls which were not thick enough to resist the lateral pressure of the water. These side walls do not appear to have been fractured, and their weight, including that of the top arch, and the earth perpendicularly above them, is considerably greater than the weight of a "full flow of water" in the aqueduct. In addition to this, is the cementing adhesion between the side walls and the bottom and of the earth outside of them. As the aqueduct resisted the force of the water pressure when nearly full, say twenty

* Page 109. † Page 107.

years ago, and from the above considerations, it is evident that the small increase now could not have produced the fractures.

The paper is useful in calling attention to an apparent failure of a portion of one of the best planned and built hydraulic works, from alleged defective design or execution, and when we have ascertained all of the circumstances which could not have produced the failure, we have almost arrived at those which could or did produce it.

The writer shows how important it is, to have stop gates in the aqueduct since it has been found necessary to draw off the water to repair the fractures which he has spoken of. The wonder is, that such gates were not put in, as soon as these accidents began to occur. Suitable gates to perform this service could have been put in, at any time during the period the water was drawn off, and thereafter would have saved the loss of half a day's present consumption of water whenever a similar accident occurred.

The suggestion that arrangements should be made to supply the distributing pipes directly from the aqueduct whenever necessary, instead of passing through the reservoir, is judicious, and is now generally adopted. It must be remembered that the great reservoir in Central Park was built for the storage of water, and that if it is only to be used to maintain the full head upon the distributing pipes, its value for storage is nearly destroyed.

In conclusion, I am of the opinion that the complete success of the Croton Aqueduct, for so many years without any failures, is the best proof that it was correctly planned and executed for the purposes for which it was designed.

Mr. Galen A. Pearsons.*—Cast iron pipes of sufficient strength to resist the whole lateral pressure of water in the aqueduct can be dropped over and secured to it, in no wise interfering with the masonry, at a cost probably not exceeding $5 per lineal foot of the aqueduct so treated.

The aqueduct can be stopped without gates, at any desired place, without difficulty, provided its strength is sufficient to resist the pressure that would bear upon the locality when stopped. This can be done by a portable apparatus, no part of which need be too heavy to be handled by one man, and can be placed in position before closing, so that no interruptions of flow will take place till desired.

I have set meters in localities where the difficulty of protection from frost, cost of maintenance and attention, was more than could be com-

* Presented April 10th, 1876.

pensated by rental. Many such localities must exist among the many thousand meters which would be required in New York. The district meter, as used in Liverpool, has induced close economy of consumption at far less cost, and has the advantage of detecting and measuring leaks in the main as well as in the service connections.

MR. JOSEPH P. DAVIS.*—In the paper under discussion, a number of matters of interest connected with the construction of water works and the maintenance of the supply are brought before the Society.

In speaking of the existing Croton Aqueduct, the writer calls attention to the fact that where carried across valleys and ravines, it has a foundation of dry rubble masonry, and that at such places there has been a lateral movement of the aqueduct masonry, opening cracks which are sources of anxiety and danger. He suggests that if the dry work had been provided with a cap of solid masonry 3 feet thick, and the side walls of the aqueduct had been made correspondingly heavy, the trouble would not have arisen. It is unquestionably true that if the foundation be made unyielding, and the side walls of sufficient weight to resist the thrust of the top arch with its load of earth and the lateral pressure of the water in the aqueduct, without aid from the earth backing, the danger of side movement will be obviated. But such construction is costly, and appears to me to be practically unnecessary.

The city of Boston, to obtain an additional supply of water, is now building a conduit leading from Sudbury river, which is to be nearly 17 miles long, and which crosses several valleys varying in depth below the grade line from a few feet to 37 feet, a few of them being several hundred feet across.

It was at first determined to carry the conduit, at these places, upon concrete arches supported upon piers of the same material, the whole covered with earth; but a further consideration of the matter, and an examination of the condition of the Cochituate Aqueduct (built 28 years ago) where on embankments, led to a change of plan, and it is now proposed to support the conduit upon carefully prepared embankments, without masonry foundation of any kind.

It was thought: *first,* that with good material and careful wetting and rolling, an embankment could be made which would be as firm and unyielding as the natural earth; and, *second,* that in case the material should not at all places prove of the best quality for compacting, still an embankment could be made which would settle but slightly, and the set-

* Presented at the Eighth Annual Convention.

tling would be uniform on any cross section (none of the embankments are on side hills—at such places I should use masonry) and would vary so gradually along the length of the bank, that there would be no breaking up of the masonry.

With a rigid foundation under the conduit, the earth back-filling, in its movements—either by settling or by the action of the weather—is drawn away from the masonry and leaves the side walls to resist alone the thrust of the covering arch and contained water.

Where the conduit rests upon the embankment itself, the whole structure settles together, if there is any settling, and the external pressure upon the side walls is increased rather than diminished.

The Cochituate Aqueduct is carried upon embankments without other foundation. One of the embankments is 50 feet high, having a depth of about 38 feet below grade line. The aqueduct is egg-shaped with the large end down, and consists of a ring of brick masonry 8 inches thick. Its internal height is 6 feet 4 inches, and its greatest internal width is 5 feet. No change was made in the cross section of the masonry where it rests on the embankments, therefore the bottom arch has no other support than the surrounding earth. There has been a change of form both on embankments and in cuts. The amount of change on the deep embankment mentioned above, will be shown by the following figures, the results of measurements made in 1874:

Station.	Dimensions. Feet.	Station.	Dimensions. Feet.	Station.	Dimensions. Feet.
106 + 75......	6.27 × 5.14	107 + 50....	6.04 × 5.24	108..........	6.04 × 5.42
107...........	6.19 × 5.21	107 + 85....	5.98 × 5.44	109..........	6.15 × 5.21

Cracks have opened in the top and bottom of the aqueduct, but there has been very little settling of the bank.

It is impossible, without cementing the interior, to build water-tight work with 8 inches of brick masonry; and even if it were possible, there will always be a sufficient change of form in such construction, after the centres are struck and the earth covering applied, to open cracks through which a considerable leakage will take place. If the embankment be made with loose material, the leakage will wash the finer particles into the interstices of the coarser, and the masonry will settle somewhat.

Such has been the action at the high embankment, and by probing through the cracks it was found that hollow spaces existed under the invert. A portion of the invert was cut out, about two years ago, and

replaced with new work; but by an examination made May 27th last, I found that cracks have opened again, and that hollow spaces have formed under the masonry as before, causing a condition of things that is both troublesome and dangerous.

The condition of the aqueduct upon other embankments is generally good; at one other, made of equally loose material there has been considerable change of form, but not more than has occurred in many of the cuts.

The high embankment consists of loose gravel and sand, and was made without rolling. It was, however, brought up in layers kept well watered, and was much traveled over with carts during construction.* Had there been sufficient binding material with the gravel, and had means to conduct away the leakage been provided, I think no harmful change would have taken place.

The following is the specification under which the Sudbury river conduit embankments are made:

"All embankments and filling below grade lines shall be made with approved material in 6 inch horizontal layers, thoroughly wet and rolled with heavy grooved rollers, or rammed in places where the roller cannot be effectively used. They shall be brought up to grade during the first season of the work, and shall be allowed to stand all winter (or until the engineer is satisfied as to their firmness), before building any masonry upon them."

A number of the embankments were raised a considerable height last season. In the fall, before frost set in, iron rods 4 feet long, each provided with a disk at the bottom, 5 inches in diameter, were set into the earth with their tops about one foot below the surface, and levels were taken upon them. At some points, wooden plugs about 2½ feet long, were driven beside the rods, to ascertain the effect of frost near the surface.

Levels have been taken again this spring which show that as a whole there has been an increase in the bulk of material, rather than a settling, as will be seen by the following:

1°. WABAN BANK.—Maximum depth of made bank, 11 feet. Bank formed with good binding gravel, requiring hard picking in the trench. It rolled to very hard and solid surfaces, differences of levels in fall and spring.

+0.031, +0.013, −0.009, +0.013 feet.

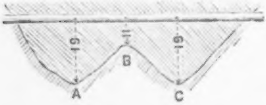
2°. FULLER'S BROOK BANK.—Maximum depth of made bank, 17 feet. Bank formed with good sand, not very coarse (good building sand), a

* In the description of the works, it is said, the embankment was formed with puddled gravel.

little gravel being used in the upper courses. The roller had not much effect in compacting. Difference of levels in fall and spring. Deepest part of bank:

+0.010, +0.002. —0.006, —0.013 feet.

3°. ALMS HOUSE BANK.—Maximum depth of made bank, 19 feet. Bank formed entirely of course sand with occasional layers of fine gravel, pea size. Difference (in feet) of levels in fall and spring:



STATION.	PLUG.	ROD.	STATION.	PLUG.	ROD.	STATION.	PLUG.	ROD.
	+0.043		B	+0.018	—0.010		0.000	—0.132
	+0.056	—0.006		0.006			+0.065	
	+0.047			—0.004	—0.011		+0.007	—0.005
A	+0.018	—0.018	C	+0.005			—0.003	
	+0.034			......			......	

4°. HURD'S BANK.—Maximum depth of made bank, 18 feet. Bank formed of loose coarse gravel with a little binding material. The original levels were taken in November, 1875; two sets were taken this spring. Difference (in feet) of levels, in fall and spring:

	Feb. 13, 1876.	March 30, 1876.		Feb. 13, 1876.	March 30, 1876.	
* { Plug	+0.042	....	Rod	+0.008	+0.008	Deepest.
{ Rod	+0.005	....	Plug	+0.038	+0.022	
* { Plug	+0.024	+0.024	* { Plug	+0.159	+0.077	
{ Rod	+0.009	+0.003	{ Rod	+0.006	+0.003	
			* { Plug	+0.028	+0.029	
			{ Rod	+0.004	0.000	

5°. WELCH'S BANK.—Maximum depth of made bank, 10 feet. Bank formed of good gravel. Difference (in feet) of levels in fall and spring:

	Feb. 1, 1876.	March 30, 1876.		Feb. 1, 1876.	March 30, 1876.
Plug	+0.084	+0.108	Rod	....	—0.010
Rod	—0.001	—0.001	Plug	....	—0.018
Rod	+0.003	—0.011			

Although the disks on the rods were set some 5 feet below the surface, it is possible they were slightly disturbed by frost.

Mr. Church's proposition to divide the pipe distribution of a large city into districts, each supplied by a separate main, is correct in principle, and would, as he says, secure a comparatively uniform distribution of water, but it does not appear to me that a complete isolation of each district—except perhaps, where there are marked differences of elevation—is either essential or desirable. No main should be tapped however, until it reaches its own district. Isolation of districts involves either a duplication of pipes in many streets or the great inconvenience and nuisance that arise from "dead ends."

I have found that where the distributing pipes in a city have been laid from year to year, in short lengths at a time and without reference to any general system, much can be done to remedy the unequal pressures that will be found to obtain, by laying sub-mains from 12 to 20 inches in diameter, and by regulating the flow by the street valves. Considerable work of this nature has been done in the city of Boston since the great fire of 1872, (when there were bitter complaints of short supply,) with very marked and beneficial effect.

His remarks upon the waste of water, every engineer who has had experience in the management of water works, will heartily indorse. The time is coming, undoubtedly, when large cities will be forced to use meters very generally, no matter how costly their introduction may be, and it should be noted that their cost cannot exceed, and in most cases will fall short of that of enlarged mains and distributing pipes required to supply useless waste.

This is a subject that cannot receive proper treatment in a short paper, I will therefore content myself with referring to my Annual Report of 1873, to show how great and how useless the night waste often is.*

* Included in Report of the Cochituate Water Board to the City Council of Boston, for the year ending April 30th, 1874.

"On July 20, observations were made at the Beacon Hill reservoir, to determine the rate of night consumption, or, more properly speaking, the rate of waste in a certain district of the city."

"This district comprises what is called the west end, north end and burnt district, and contains not far from 60 000 inhabitants. In it are located many of the manufacturing houses, principal hotels, newspaper offices, printing-houses, etc., of the city; but at the time selected for the experiment, between twelve and three o'clock, Sunday morning, the legitimate use of water must have been very small."

"This section was shut off from all communication with the Brookline and Chestnut Hill reservoirs, by gates on Bedford, Washington, Tremont, Charles and other streets, and fed exclusively from Beacon Hill reservoir. The leakage through the gates, if any, must have been inappreciable, as the pressures on opposite sides could have differed but slightly."

Mr. J. James R. Croes.—The paper under consideration, and the remarks, it has called forth, contain much that is worthy of serious thought.

I. It is shown that aqueducts which depend for stability upon a combination of earth work and masonry, neither of which alone is of sufficient strength to resist the pressure of the full flow of water, are not durable. The Croton, the Cochituate, and the Washington Aqueducts have cracked longitudinally under these conditions.

These cracks occur only on embankments, and it is stated that each longitudinal crack is usually preceded by a transverse crack at the ends of the embankment. This has occurred, as well when the masonry rested on a base of rubble stone laid dry (as in the Croton Aqueduct), as when it rested on rammed earth alone (as in the Washington Aqueduct).

The primary cause is, therefore, insufficient compacting of the supporting embankment. This accounts for the transverse cracks.

The longitudinal cracks arise from the lateral yielding of the conduit or tube, under interior pressure. The masonry settles away from the superincumbent earth, which being well rammed, is compact, and on which there is not sufficient pressure to break the bond it has in itself and cause it to follow the masonry, which has a smooth plastered exterior surface. The masonry relieved of external pressure is split.

The bond between the earth and masonry being once broken, every slight change of relative position of the two masses, whether from addi-

"Observations were commenced at midnight, and readings of the gauge taken every 15 minutes. At the first of the experiments the consumption was found to be somewhat irregular, but between one and three o'clock it was remarkabiy uniform, showing that the draft was not due to irregular opening and shutting of cocks, but to a continuous flow at almost unvarying outlets."

"There were drawn from the reservoir during these two hours, 386 857 gallons, equal to a rate of 4 642 284 gallons in 24 hours. This enormous rate of night consumption indicated either a heavy leakage or great waste. A party of inspectors was at once organized, under the direction of Mr. Joseph Whitney, of Cambridge, who, from experience gained on the Cambridge Water Works, was particularly qualified for this work, and a careful inspection of all the fittings in the district was made, and the street mains were tested for leaks in various ways. No leaks were discovered in the mains, but many hundreds of defective fittings were found and repaired, and some leaks in the house service-pipes detected and stopped. Before the examination was concluded, however, it became manifest that much the greater portion of the night consumption was caused by waste; that is, by flow through open fittings. All the leaks that could be discovered having been stopped, a second observation was made on Sunday morning, October 5th; between the hours of twelve and three, as before. The water in the reservoir at the commencement of the tria! stood at the same height as on the morning of July 20."

"There was a slight wind blowing at the time of the latter trial, which caused an oscillation in the gauge tube, and the readings were not so satisfactory as those of July. During the three hours of observation the water fell 2 feet 4$\frac{5}{8}$ inches, showing a consumption of 506 182 gallons, which is at the rate of 4 049 456 gallons in 24 hours. The consumption between one and three o'clock was 336 294 gallons, or at the rate of 4 035 528 gallons in 24 hours, showing a small saving, about 13 per cent., caused by the repairs made."

Note.—The Beacon Hill reservoir is a stone structure, supported at a considerab!e height above the street level, on arches. As it has a small water area and nearly vertical sides and as any leakage can be readi!y detected, it forms an excellent measuring tank.

tional settlement of the base or from variations of temperature which affect the ground, renews or enlarges the crack in the masonry.

The only effectual remedy for such leaks, seems to have been the increasing of the thickness and weight of the covering. In cases on the Croton Aqueduct where the outside protection walls have been raised and strengthened, and on the Washington Aqueduct where the earth bank has been raised and widened, the leaks have ceased.

The recent removal of portions of the Croton Aqueduct on embankment, within the city of New York, has afforded an excellent opportunity for observing the extraordinarily good character of the masonry laid forty years ago, which we would do well to imitate, and the portions of the design which we should be careful not to follow.

Across a valley 2 000 feet long, the aqueduct was carried on an embankment, which at its greatest height is 50 feet above the surface. The exterior width of the aqueduct masonry at bottom is 12 feet 1 inch. This rests on a bearing wall of large unwrought stone laid up dry in 2 feet courses, with the interstices filled with small stone. At the bottom of the aqueduct the structure is 30 feet wide, the face walls being of stone with dressed joints laid in mortar for one foot from the face. These walls have a face batter of 1 in 12, and are carried up to within 14 inches of the crown of the arch. The space between these walls and the foundation and aqueduct is filled with dry rubble up to 2 feet above the bottom of the aqueduct, and above that with earth which is continued to 4 feet over the crown, the bank at the top being there 8 feet wide, and having slopes of 2 to 1.

The effect of time on this structure has been as follows :—Where the bank is 40 feet high, the dry stone foundation has settled, the aqueduct has split longitudinally at top and bottom, the earth has worked down among the loose stone, the face wall on each side has spread 1½ inches at the base, 2½ inches at bottom of the aqueduct, and has fallen in 2 inches at top. Where uneven settlement of the foundation occurred, the concrete base of the aqueduct is in places unsupported for several feet.

The suggestion made by one member,* that the failures may be chargeable to misapplication of the structure to purposes for which it was not designed, is not sustained by facts, nor even by theory.

The practical conclusions to be drawn from the facts stated in the first part of Mr. Church's paper and the discussions thereon, are :

1°. The support or foundation, and the covering of a masonry conduit should be of homogeneous material, sufficiently compacted to pre-

* Page 261.

vent any but very slight settlement. Either solid masonry or well rolled and rammed earth embankment fulfil these conditions. A dry rubble stone support and earth covering do not fulfil them.

2°. The conduit itself must either be strong enough to resist the thrust of the full flow of water, or must be covered to such depth that there will be below the point to which the action of the frost extends, an amount of covering material, sufficient to supplement the deficient resisting power of the masonry.

3°. It appears that both on the Croton and Cochituate Aqueducts, serious difficulties occur in consequence of cavities under the conduit, caused by leakage. This shows the necessity of some system of under drainage on embankments.

If bottom cracks what drainage ?

II. The question is asked several times in the discussions : why stop-gates have never been put in, as suggested by Mr. Church ? To this no reply has been made. Possibly a reason for this omission may be found in the fact that, in the management of New York finances, the appropriations asked for by engineers are annually revised by non-professional Commissioners, readjusted by a Board of Apportionment consisting of four non-professional officials, amended by an elected Board of Aldermen, and finally settled by the Board of Apportionment. By the time the estimates for "aqueduct repairs and improvements" for the ensuing year have passed through this ordeal, they are generally reduced to a sum barely sufficient for only ordinary expenses. There may be, however, other reasons which are not known, but it does not seem possible that among them should be an idea that the value of such gates would not equal the cost.

III. The suggestion made regarding arrangement of gate-houses in the reservoir, so as to feed the mains direct from the aqueduct, appears to be good, and would doubtless be efficacious in keeping up the head in the higher parts of the city, when used in connection with the suggested division of pipe districts. It is stated that the question of such an arrangement was discussed when the gate-houses were planned in 1857, and decided adversely, on the ground of excessive expenditure, and as likely to be unnecessary for many years to come. The same misapprehension of the probable increase of consumption, which in 1848 caused the laying of pipes of too small size across the High Bridge, seems to have existed in this case.

IV. The prevention of waste appears to be the most difficult problem to handle, for it involves a supervision of the fittings and their operation

in private houses. The only method of checking this lavish waste is by meters. The main argument against them seems to be that their introduction will encourage the disuse of water. With, however, a fixed minimum rate, as in Providence, this argument has little weight. The waste is among the wealthier classes rather than among the poorer. The adoption of meters would not obviate the necessity for undertaking, at the earliest possible time, the construction of a new aqueduct and additional storage reservoirs. This summer's drought has exhausted the present storage facilities. A second reservoir is fortunately approaching completion. A third should be begun at once.

The concluding sentence of Mr. Church's paper is calculated to mislead. He says that fortunately, the Croton river offers unlimited supply for all future demands. On the contrary, the utmost that the Croton river can be depended upon to supply, even with a complete system of storage basins retaining every particle of water which the stream yields, is 33 700 000 cubic feet per day. The present consumption is stated to be 15 200 000 cubic feet. The annual rate of increase of consumption has been, according to official statements, 5.8 per cent. since 1863, in which year I find the first record of the consumption from actual experiments.

At that rate the limit of the Croton river will be reached in fourteen years. If by means of meters, the present consumption should be reduced to only 8 000 000 cubic feet per day, the rate of increase would not vary from that which has prevailed. The limit of the Croton river's yield would in that case be reached in twenty-six years. Taking into consideration the difficulties of repressing waste and of storing all available yield of the streams, the enormous additional pipeage required for supplying the more scattered population of the suburban wards, which are now becoming settled, my own impression is, that the shorter period is more likely to be the real one, and that this century will see an additional supply of water introduced into New York from another source than the Croton.

Mr. Benjamin S. Church*.—The object of the paper under discussion was to suggest certain additions and changes in the gate houses and pipe distribution whereby repairs of the aqueduct would be facilitated, and dangers, present and future, averted. In endeavoring to set forth the urgent necessity of metering the city as the only successful way of stopping reckless waste of water that imperilled the daily supply, it was

* Presented, to conclude the discussion.

necessary to state the condition of the aqueduct, and briefly as possible, furnish facts upon which the opinions were based.

To the surprise of the writer, main attention has turned upon that prolific source of discussion in the profession—settlement of foundations causing cracks in masonry.*

The effect of frost in relieving the sides of the conduit of part the pressure of earth spoken of by Mr. Francis † was suggested by the writer many years ago in answer to inquiries of Mr. Croes, then in charge of the Washington Water Works, as to the cause of cracks in the Croton Aqueduct. When the embankment is in a position for the sun to concentrate on one side, frost penetrates to a greater depth on the part constantly shaded, thus producing a still more serious effect in disturbing the balance of pressure.

Mr. Hutton is correct in the surmise that in light cuttings, on good foundations, the cracks are wanting.

Mr. McAlpine's statement ‡ that the Croton Aqueduct has been followed "even in detail of plan and execution" on all similar works in the United States, is inaccurate. His reasoning and conclusions from consideration of the weight of the aqueduct and water contained therein, also superincumbent earth, are hardly applicable, as the writer's observation § referring to the minute but constant motion in foundation walls was not dependent alone upon the added weight of the aqueduct. Thus the conclusion of his argument does not logically follow "that the longitudinal fractures described have *not* been produced by the settlement of foundation walls, or by crushing of stones of which they are formed.

Mr. McAlpine's statement respecting the variations of the water pressure,‖ viz., "this almost constant load, almost equally distributed over the top of these well bonded walls, would have no tendency to spread the walls" is true; but the spreading of the walls on which the aqueduct masonry rests unquestionably has a tendency to split the masonry, which the pressure of the water as surely augments. His remark that "as the aqueduct resisted the force of the water pressure say for twenty years ago, and from the above considerations it is evident that the small increase now could not produce the fractures is very true," but it can and does frequently reopen the fractured masonry. His concluding sentence that "the complete success of the Croton Aqueduct for so many as

* The writer desires to acknowledge the valuable data and results of experience given by Messrs. Shedd, Francis, Hutton, Davis, Croes, etc., who corroborate his experience of the past seventeen years, as Engineer in Charge of the Croton Aqueduct Repairs and Maintenance.
† Page 258. ‡ Page 260. § Page 107. ‖ Page 263.

twenty years ago without any failure," is not borne out by the records of the Department prior to that date.

A serious objection to dry foundation walls as a support for aqueducts, when enclosed by earth embankments, is that they act as blind drains, collecting earth soakage water into themselves and retaining it, when there is no provision for its escape, and when they rest upon earth, softening that also, causing settlement, as well as tending to promote disintegration. Any leakage from the conduit finds its way into the foundation walls as well.

Mr. Pearson remarks "that the aqueduct could be stopped with gates at any desired place without difficulty, provided its strength is sufficient to resist the pressure that would bear upon the locality when stopped." The Croton Aqueduct was not designed to bear such pressure, and wastage ways are therefore indispensable in conjunction with the cross gates.

It should be said, before closing, that the daily consumption of water given in the paper, includes in addition to the amount passing through the aqueduct, which was at that time about 105 000 000 gallons, that which was being drawn from city storage in the reservoirs which were then falling.

ON IMPROVEMENT OF THE MOUTH OF THE MISSISSIPPI RIVER.*

Mr. Elmir L. Corthell.—It may be of interest to state that this day, one year ago,† work was commenced on the South Pass jetties. The contractor, James Andrews, of Alleghany City, Pa., arrived the day before with a stern wheel steamboat, a pile driver, and a few workmen. They landed among the reeds, and broke down the tall grass along the banks to find a place for their anchor in the soft mud. There was no house, but the light-house, within several miles, and with swarms of mosquetoes and sand flies and the oppressive heat, the commencement of the work was anything but auspicious. On June 15th, the first pile was driven for the wharf at Lands End, and the work of preparation and of actual construction went forward rapidly.

It is my privilege to give you the results that have been reached in the twelve months intervening between that day and this. I am glad to find, by personal conversation with members, that there is a very great interest in this work, and as the generally expressed wish is for facts, I

* Referring to CXIV, Notes on Improvement of the Mouth of the Mississippi, W. Milnor Roberts; Transactions, Vol. IV., page 32. † June 15th, 1875.

will confine myself to them entirely, and leave the question with you, as to whether they are favorable or otherwise to the ultimate success of the jetties. Having been the engineer in charge, my peculiar work has brought me into contact with facts and actual conditions; and the rapid progress of the work, with the consequent arduous field and office duties, have given me but little time to analyze or theorize on the facts brought daily under my notice.

Considering the fact that this question of jetty construction, as applied to the mouth of the Mississippi, has been discussed fully, not only in engineering circles, but also by the general press of the country, and in pamphlets and Congressional papers, until the nation, as a whole, is familiar with the various theories advanced to prove and disprove the success of the enterprise; it is not my place to weary you with a discussion of theories, but I will proceed at once, after a brief explanation of the work to be done, to present what facts I have.

The Mississippi river, when within about 12 miles of the Gulf, separates into three rivers or passes, and thus forms the Delta. The three passes are A'Loutre, South and South-West. The South Pass is the smallest of the three, though the central one. The volume of water carried to the Gulf through it, is but 12 per cent. of the whole volume of the river. South-West Pass carries about 58 per cent., and Pass A'Loutre, with the smaller passes flowing out of it, carries the remainder.

A shoal bar exists at the mouth of each of these three passes, the depth of water on each varying generally in proportion to the volume carried to the Gulf over it. This bar is composed entirely of sedimentary matter brought down by the river. The water issuing from the passes, no longer confined by banks, spreads out on either side. The velocity diminishes, the sediment drops, the bar forms. The central thread of the current being the strongest, and the water being the deepest there, the velocity is preserved and the sediment carried out much further than in the shoal water over the submerged new banks of the pass. The outer crest of the bar is thus thrown out 2¼ miles from the land's end at the South Pass, and 5 miles at the South-West Pass. The depth on the bar at the former pass was 7½ feet at mean low tide, and at the latter it is 15 feet.

The principle to be applied to deepen the channel through the bar was a concentration of the volume by means of parallel jetties or dykes. Whether its application will be likely to produce the required depth, viz.: from 20 to 30 feet below average flood tide, you can judge from the facts to be presented.

It was doubted by some whether the material composing the bar, being freshly deposited, would have sufficient solidity to uphold works of the requisite weight and strength to resist the servitudes of storm, waves and river currents. The following table will give a better idea of the character of the jetty foundation than any description.

TABLE I.

Showing Character of Foundation of Jetties from Record of Pile Driving.

Piles from 8 to 20 feet apart. Distances in Table are from Lands End, East Side; piles 10 inches at small end and 14 inches at large end.

Location of Piles.	Weight of Hammer.	Average fall of Hammer.	Average of Blows.	Average depth Driven.
East Jetty.	Pounds.	Feet.	Number.	Feet.
From 4 200 feet to 5 500 feet...... " 7 200 " 12 100 "	3 000	15.5	44.0	16.7
" 9 100 " 9 3:0 "	3 000	18.6	98.0	17.4
" 11 900 " 12 100 "	3 000	19.0	45.0	28.2
West Jetty.—Whole length of same........	3 000	19.5	79.7	19.5

Analysis of number of blows for different depths, approximate—

Number of blows	first	5	feet driven,	4,	or 15	inches	per	blow
" "	next	6	"	20,	or 3.6	"	"	"
" "	"	4½	"	20,	" 2.7	"	"	"
" "	last	4	"	35.7,	" 1.3	"	"	"

Average distance driven at last blow, ¼ inch.

It will be seen from this table that at about 9 300 feet on the east jetty, the material is much harder than elsewhere, and the detailed record of the pile driving on the west jetty shows the same material to exist there, at the same distance. The channel on the line of this hard material resisted much longer than that of any other locality the excavating power of the current.

The following table illustrates the progressive deepening towards the outer bar. The contours refer to the plane of average flood tide as established by Maj. C. B. Comstock, who is inspector of the work for the United States government. The distances in all the tables are from a common zero point at Land's End at the commencement of the east jetty, and from a triangulation station named East Point, established by Mr. Marindan of the U. S. Coast Survey, whose charts, made from a survey

terminating in May, 1875, are the basis on which our location was made, and which we still use for comparative purposes.

TABLE II.*

Record of Distances in Feet through the Bar between the Contour Lines.

Contour.	May, 1875.	Dec. 1875.	Jan'y. 1876.	Feb'y. 1876.	Mar. 17, 1876.	Mar. 29, 1876.	April, 1876.	May, 1876.	July 30, 1876.	Aug. 1876.
12 Feet.	4 305	2 635	750	0	0	0	0	0	0	0
15 "	5 925	4 400	4 175	2 175	450	50	0	0	0	0
18 "	7 000	5 692	4 480	3 960	2 575	1 180	750	175	0	0
20 "	9 635	6 215	5 425	4 490	3 255	2 955	2 200	720	75	0
22 "	....	....	....	....	....	5 270	3 995	1 952	1 350	975
24 "	....	....	....	....	....	7 600	5 975	3 120	2 750	2 550

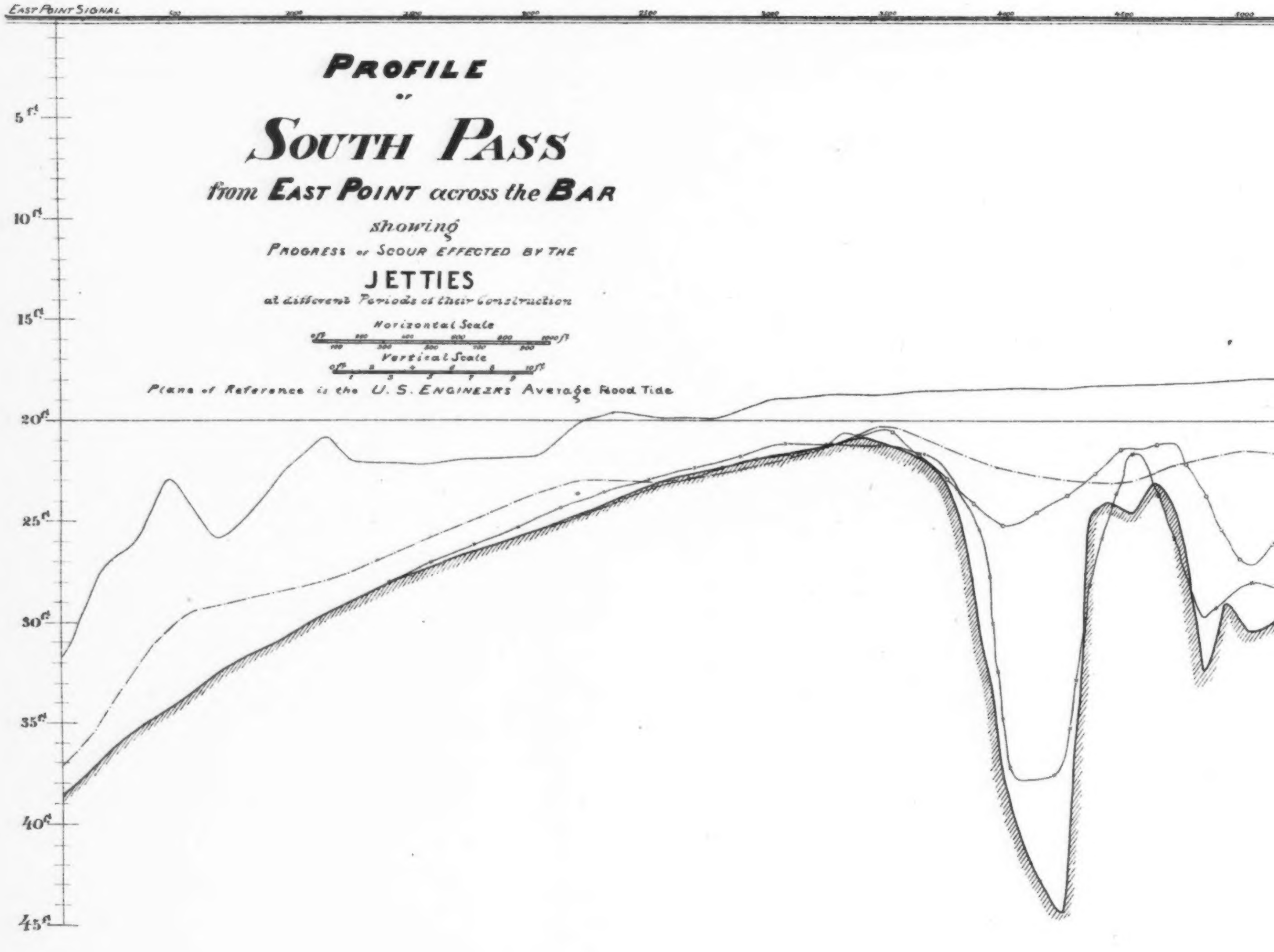
No record of the 22 feet and 24 feet contours was made prior to March 29th, 1876.

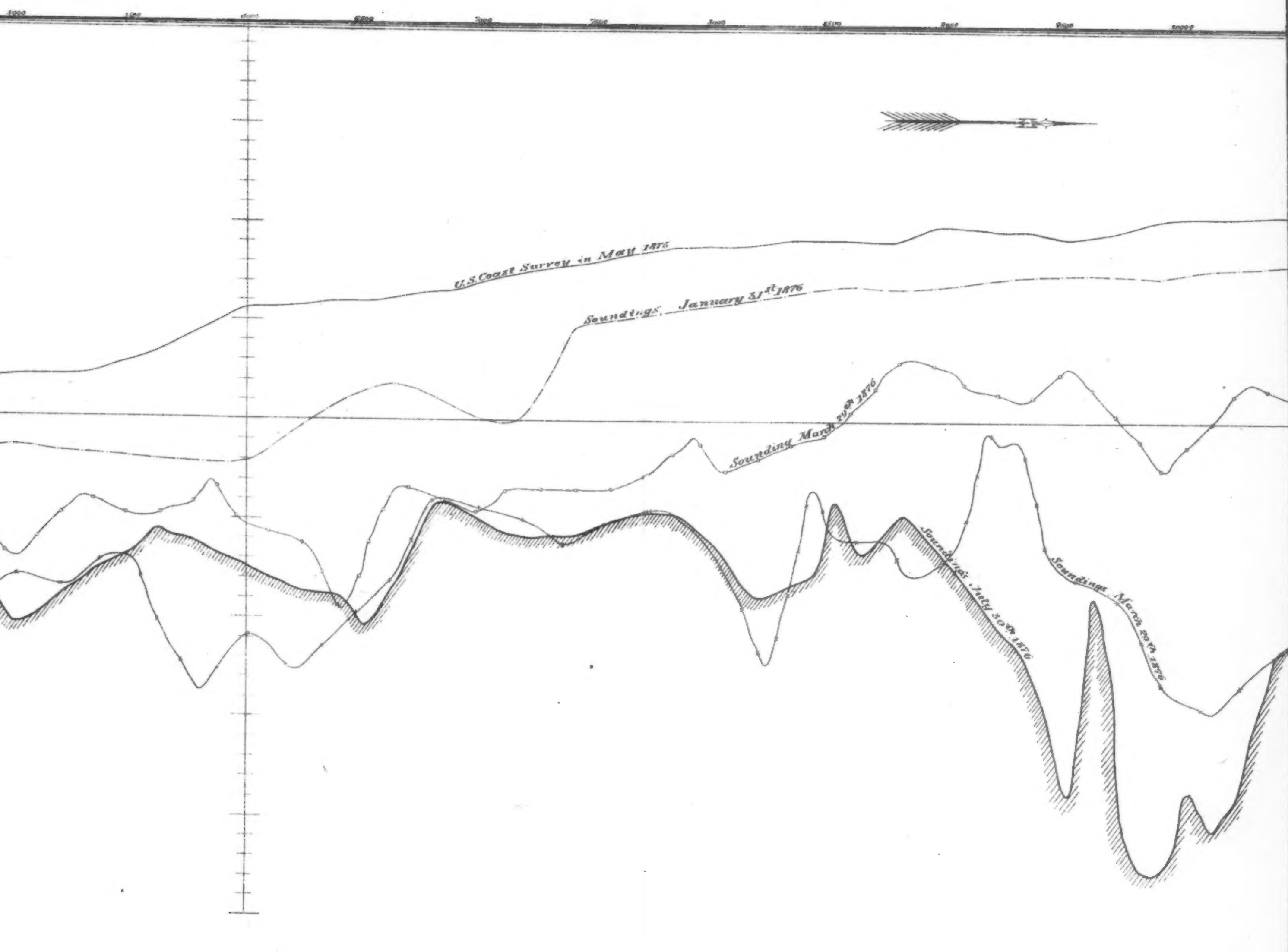
These distances embrace all portions of the bar that rise above the contours between the 35 feet depth at Land's End and the deep water outside the bar. The progressive reduction in the size of the bar is generally in comparison with the progress of the jetties, especially at the earlier dates. The surveys from which this and tables following are compiled are made in the most thorough manner, and with no effort spared to render the surveys accurate and the result reliable.

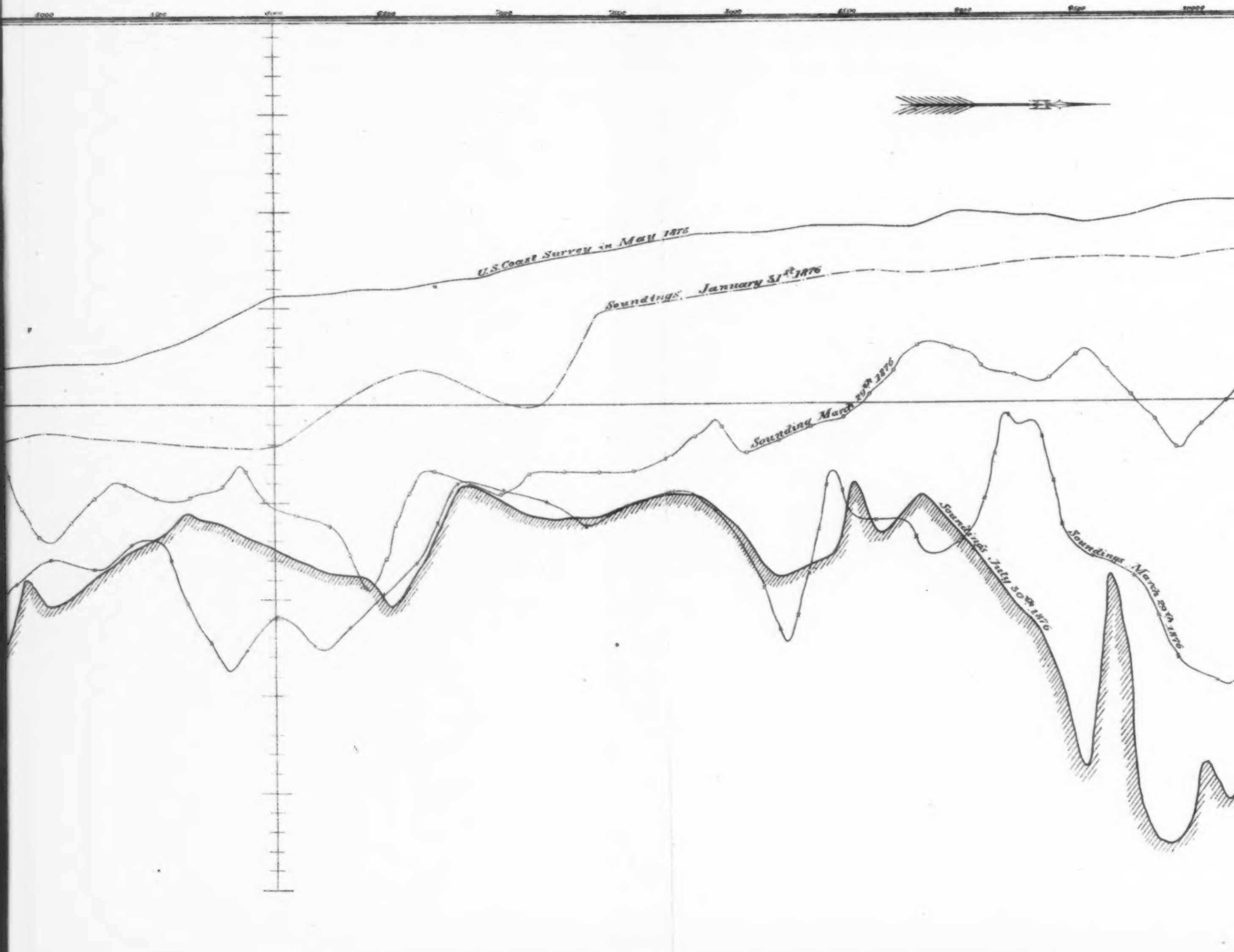
The deepening has been affected by the operation of the river current and by the tides, although the latter in this portion of the Gulf are diurnal and feeble, having an oscillation of 22 inches only, between mean low water and mean high water, yet at the season of highest and lowest tides the scouring power of the current is considerably augmented.

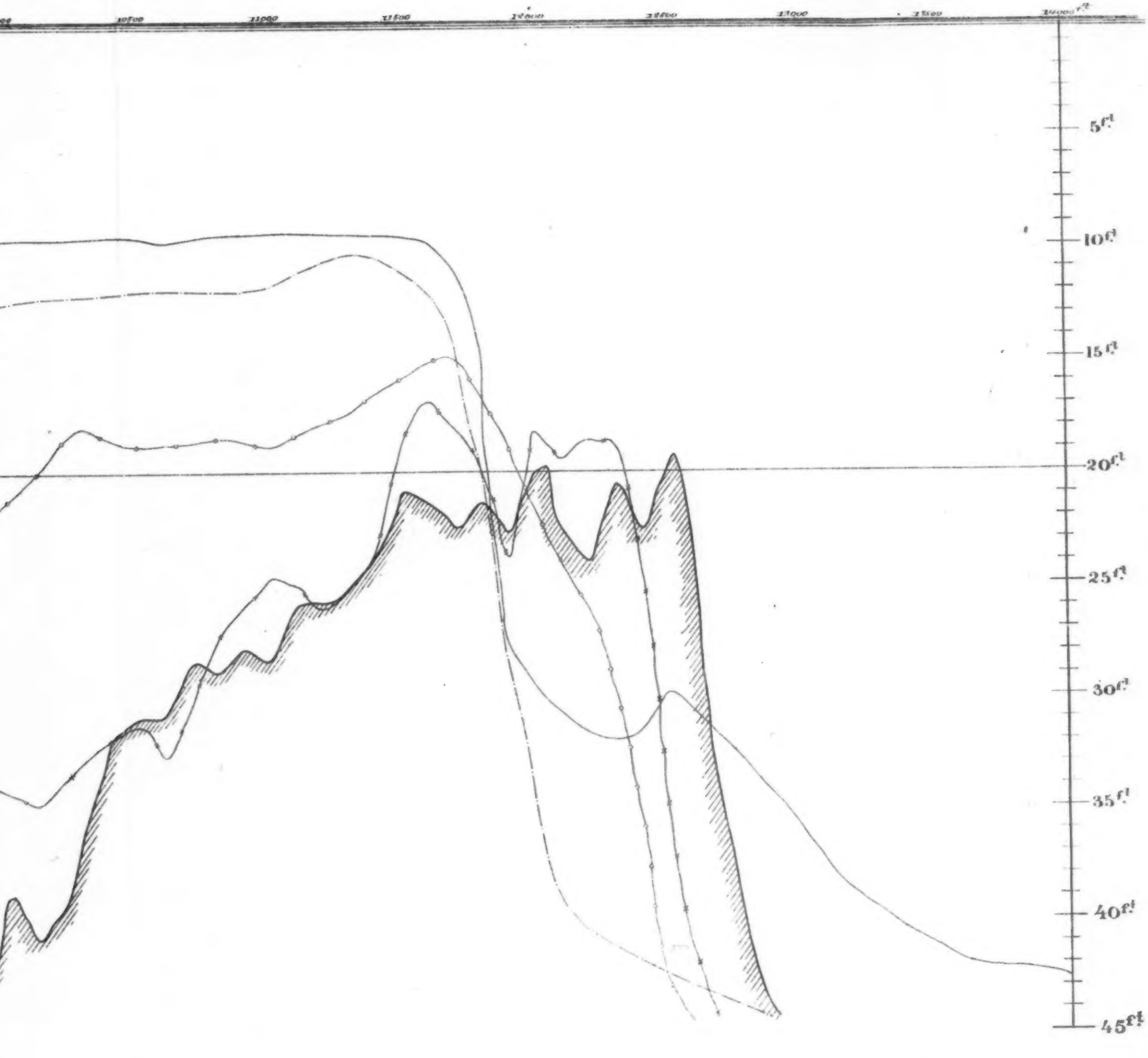
Tables III, IV (pages 279, 280), show the progressive depths in the line of deepest water. It will be noticed by an examination of these tables, that there are irregularities in the progresive depths and widths, all of which explain themselves, when the exact condition of the jetty construction is known and the manner in which the channel is made by the current is understood, Plates I and II annexed will assist in further explaining the process of enlarging the section. It will be noticed that the attempts of the concentrated volume to excavate a channel were peculiar and at times spasmodic, that a desperate attempt to deepen the

* This and following tables have been enlarged to include results up to August 14, 1876.


EAST POINT SIGNAL
PROFILE
OF
SOUTH PASS
from EAST POINT across the BAR
showing
PROGRESS OF SCOUR EFFECTED BY THE
JETTIES
at different Periods of their Construction
Horizontal Scale
Vertical Scale
Plane of Reference is the U. S. ENGINEERS Average Flood Tide
5 ft.
10 ft.
15 ft.
20 ft.
25 ft.
30 ft.
35 ft.
40 ft.
45 ft.

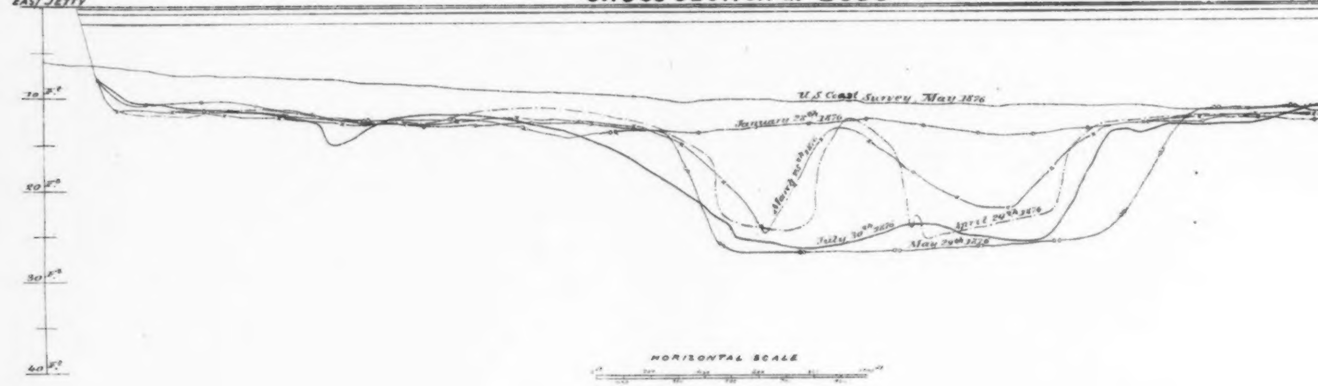

9000
9500
10000
U.S. Coast Survey in May 1876
Soundings January 31st 1876
Sounding March 29th 1876
Soundings July 30th 1876
Soundings March 29th 1876


10000
U.S. Coast Survey in May 1875
Soundings January 31st 1876
Sounding March 29th 1876
Soundings July 30th 1876
Soundings March 29th 1876

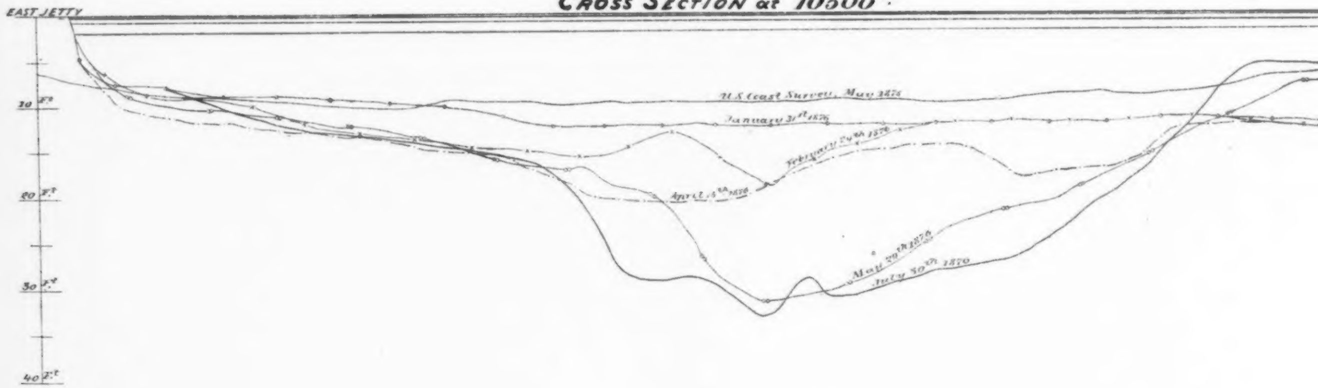


11000
11500
12000
13000
5ft.
10ft.
15ft.
20ft.
25ft.
30ft.
35ft.
40ft.
45ft.

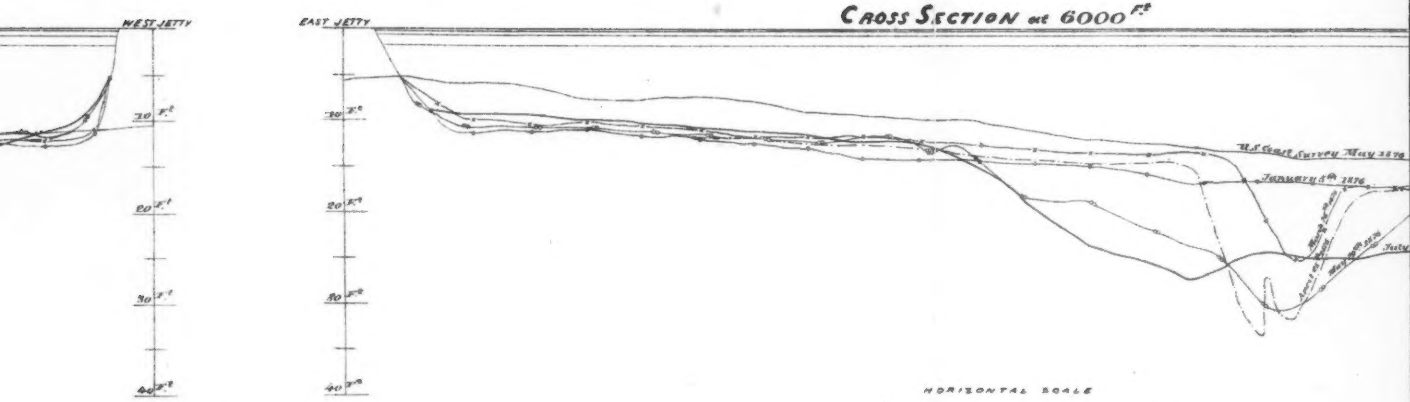
CROSS SECTION at 8000 Ft



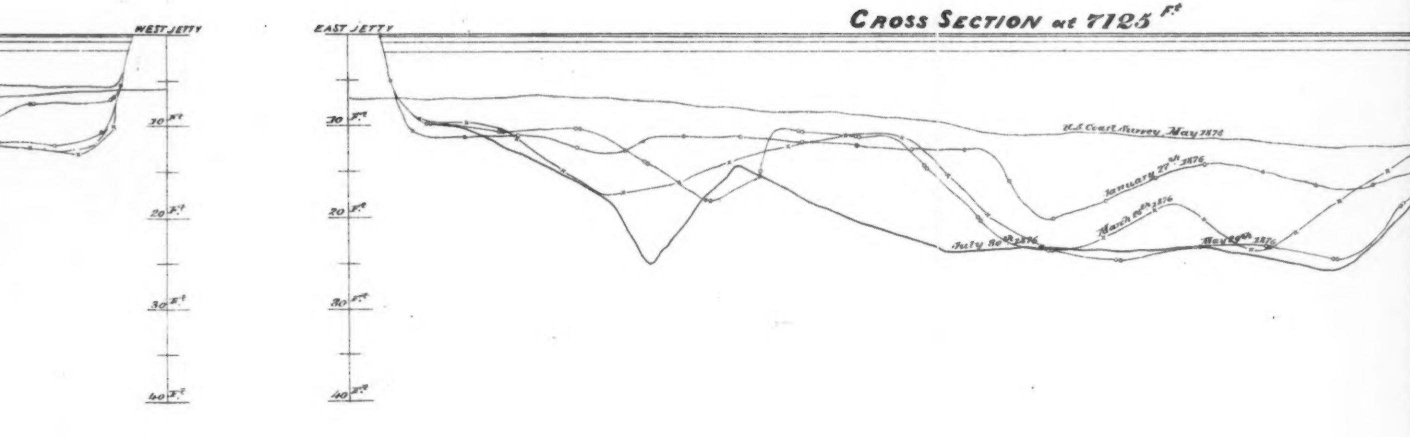
CROSS SECTION at 10500 Ft



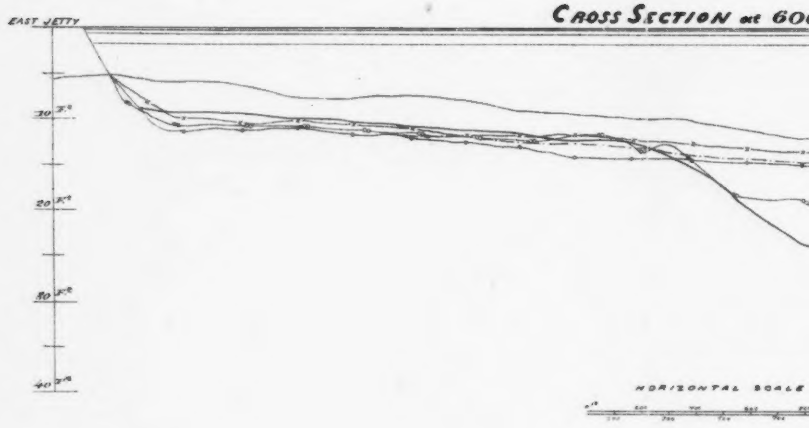
CROSS SECTION at 6000 Ft

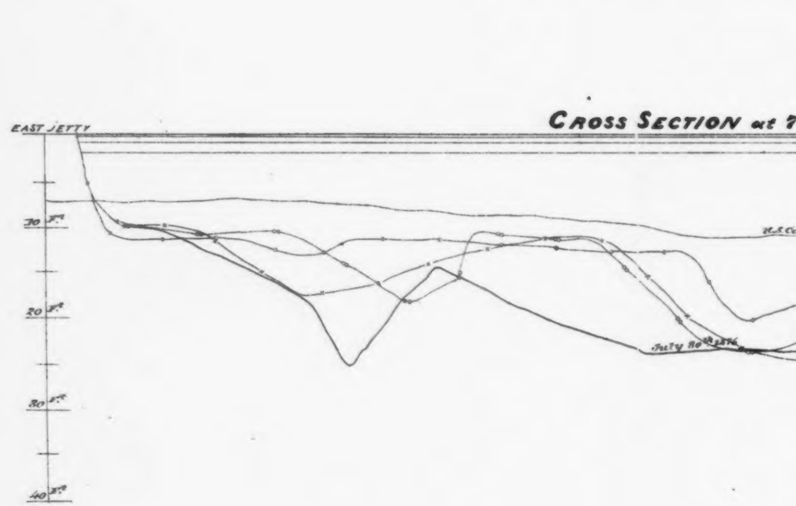


CROSS SECTION at 7125 Ft

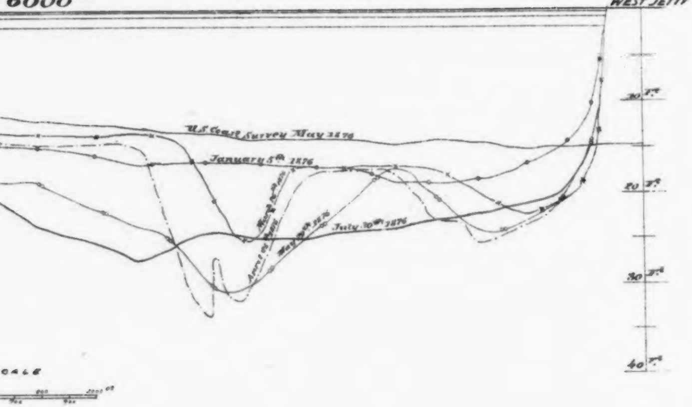


AM. PHOTO-LITHOGR

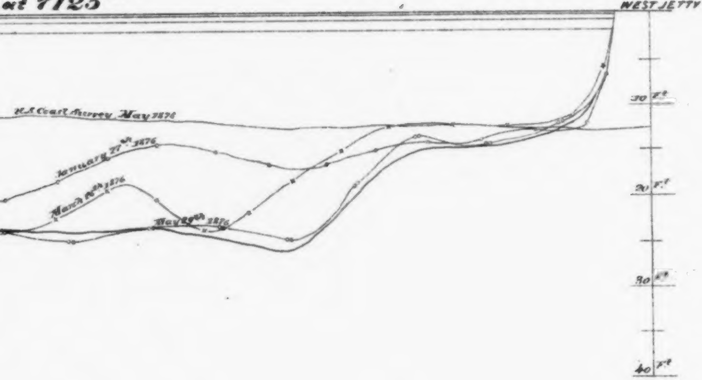

CROSS SECTION at 60
EAST JETTY
20 Ft
40 Ft
HORIZONTAL SCALE


CROSS SECTION at 7
EAST JETTY
20 Ft
40 Ft

6000 Ft



at 7125 Ft



AM. PHOTO-LITHOGRAPHIC CO.N.Y. (OSBORNES PROCESS.)

TABLE III.

Depths (comparative) in Line of deepest Water, in Feet.

Distances from E. Point.	May, 1875.	Dec., 1875.	Jan'y, 1876.	Feb. 1876.	March, 1876.	April, 1876.	May, 1876.	June, 1876.	July, 1876.	Aug. 1876.
	31.8	37.0	37.0	38.0	38.7	38.0	37.5	37.1	37.1	....
1 000	21.0	28.3	28.0	28.0	28.6	28.0	28.5	28.7	29.0	....
2 000	21.8	22.8	23.0	23.0	23.2	25.0	23.9	24.0	25.0	....
3 000	18.8	22.5	22.0	21.8	21.2	22.3	21.4	21.9	21.6	....
3 500	18.0	20.0	20.0	20.0	20.2	21.8	21.4	21.9	21.1	....
4 000	18.0	22.3	22.4	22.5	27.4	28.8	37.9	38.5	39.5	....
4 500	18.0	23.3	22.9	23.5	22.9	23.3	21.5	23.3	30.3	....
5 000	17.9	23.0	21.4	28.5	25.4	27.1	28.0	28.2	30.3	....
5 500	16.0	19.8	21.4	23.3	24.9	25.1	27.0	25.5	26.8	....
6 000	14.3	18.8	22.2	22.6	25.4	38.4	30.6	29.1	27.3	....
6 600	14.0	16.2	18.1	17.2	23:2	24.2	28.1	30.1	26.8	....
7 000	13.0	15.6	20.1	24.5	23.4	24.0	24.4	26.0	26.0	....
7 484	11.8	13.5	14.6	24.0	22.5	21.8	24.7	27.4	24.9	....
8 000	11.0	13.0	14.1	18.0	18.0	22.4	26.9	28.3	26.4	....
8 200	11.0	13.0	14.5	18.0	18.2	28.0	32.4	30.3	28.9	....
8 500	10.9	13.3	13.1	15.0	20.4	23.2	25.8	27.6	23.9	....
9 000	10.5	11.8	13.0	15.0	18.0	23.2	26.9	26.8	27.6	....
9 500	10.8	10.8	11.0	13.0	17.4	20.3	47.3	45.0	38.8	....
9 900	10.0	11.0	12.6	14.0	16.0	22.8	33.9	34.0	42.8	....
10 500	9.5	9.3	12.0	16.6	18.7	19.8	32.7	31.0	30.8	....
11 000	9.2	9.8	12.0	14.6	18.0	19.2	24.5	26.2	28.3	....
11 500	9.2	11.3	12.0	15.0	15.9	18.2	22.7	21.5	20.8	23.5
11 700	9.2	16.8	17.0	18.0	15.0	17.0	18.0	21.0	22.4	22.5
11 900	11.8	15.0	18.0	18.5	19.0	18.0	16.5	18.3	22.8	23.0
12 000	20.8	40.8	34.2	13.2	20.8	23.0	30.2	30.5	20.0	23.0
12 400	31.0	40.0	35.0	30.0	47.0	42.3	25.0	20.0	22.8	21.0
12 800	35.0	....	....	....	....	....	....	....	41.8	39.7

channel would result in a channel too deep for the necessities of the case, and a widening taking place soon after, the depth would become less. It may be taken as a general fact that an unusual and extraordinary deepening would result in a widening and a subsequent shoaling, and that a narrowing of the channel would indicate a deepening.

The peculiar condition of the work also tended to produce irregularities; for instance, when the dam connecting the upper end of the west

TABLE IV.

Record of Width in Feet, of Channel at a Depth of 20 Feet, on the established Cross Sections and at different Dates.*

Location of Cross Section.	May, 1875.	Dec. 1875.	Jan'y, 1876.	Feb'y, 1876.	March, 1876.	April, 1876.	May, 1876.	June, 1876.	July, 1876.	Aug. 1876.
E.Point.	408	553	553	493	500	520	530	540	540	....
1 200	430	470	470	441	460	460	450	440	440	....
2 200	230	492	492	480	433	470	430	410	465	....
3 000	—	325	325	212	300	470	410	350	470	....
3 500	—	300	300	220	285	480	470	300	200	....
4 000	—	30	305	312	366	362	340	380	370	....
4 500	—	110	260	165	300	345	343	300	290	....
5 000	—	—	305	167	165	205	202	140	170	....
5 500	—	—	165	207	184	205	266	205	225	....
6 000	—	—	82	75	122	212	308	340	450	....
6 612	—	—	—	65	190	280	338	315	350	....
7 125	—	—	10	215	210	326	320	320	370	....
7 584	—	—	—	145	155	260	330	340	390	....
8 000	—	—	—	17	118	192	308	350	325	....
8 500	—	—	—	—	—	90	338	340	395	....
9 000	—	—	—	—	—	64	295	380	380	....
9 300	—	—	—	—	—	—	105	270	410	....
9 900	—	—	—	—	25	38	203	300	360	....
10 500	—	—	—	—	—	—	300	280	385	....
11 000	—	—	—	—	—	—	208	349	400	....
11 500	—	—	—	—	—	—	100	120	335	500
12 100	400	1 000	640	320	868	—	217	200	385	725

jetty with the shore was built, there was a head of water produced against it, extending to the east jetty, but diminishing as it approached the latter. This condition produced quickly a deep channel below the line of the dam, and the channel above the dam had a tendency to shoal slightly, caused by the reduction of velocity which the head of water had produced.

The deep channel immediately below the line of the dam caused a temporary shoaling at a point about 500 feet below the dam, as the velocity for the first 300 feet was too great to be maintained. A comparison of the bottom velocities above and below the dam, as ascertained imme-

* A dash in the columns denotes less than 20 feet depth at the date where it occurs.

diately after its closure, will further illustrate the subject. The dam is located 4 000 feet from East Point, and nearly at right angles to the jetty lines. Above 4 000 feet, the velocity was 2.2 feet per second; from 4 000 feet to 4 500 feet, 2.86 per second, and from 4 500 feet to 5 000 feet, 2.31 per second.

The varying conditions of the river and the amount of sediment carried in suspension, the difference in the material composing the bar at various places, the state of the tides, the direction and force of the winds, the storms and resulting seas that rolled in against the current, the conditions of the works at Grand Bayou and at the head of the pass, the state of the jetties especially—all conspired to render the work of channel excavation irregular, but taking the general progress into account the advance in depth and width has been constant and steady, and the effect of the jetty construction has shown that the volume of water issuing from the pass will eventually recover the section it has normally above.

A dam has been thrown across Grand Bayou, and about 90 per cent. of its volume turned into the pass. It is reasonable to expect that the section between Grand Bayou and the sea end of the jetties will enlarge until it is as large as that above Grand Bayou, or to an ultimate section of 25 000 square feet, it now being 15 000 square feet. (Reference is made to Table V, following.) It is also reasonable to expect that the section between the jetties will finally be larger than the section above Grand Bayou, as a larger volume will necessarily flow between the jetties during the ebb of the tides than flows through the pass above Grand Bayou, as the range of the tide is greater and the slope steeper near the gulf than it is 8 miles above.

TABLE V.

Comparative Areas of Sections of South Pass.

Location of Section.	Plane of Reference.	Area in Sq. Feet.	Date of Survey.
One mile below Head of Pass.....	Mean high water.........	24 395.0	U. S. Coast Survey, May, 1875.
	of U. S. Coast Survey,	25 097.3	Jetty Eng'rs, Aug. 2d, 1876.
3 600 feet above Bayou Grande....	Mean high water.........	23 243.0	U. S. Coast Survey, May, 1875.
	of U. S. Coast Survey,	26 037.0	Jetty Eng'rs, May 17, 1876.
South Pass Light House..........	Average flood tide	16 711.0	U. S. Coast Survey, May, 1875.
	of U. S. Eng'rs	17 006.0	Jetty Eng'rs, July 30, 1876.
5 500 beyond Lands End and between the Jetties; present depth, maximum 27 feet	Average flood tide........	14 804.0	Jetty Eng'rs, July 30, 1876.
	of U. S. Eng'rs	....	....

But the facts relative to final sections and volumes and velocities cannot be obtained at present, during the formative state of the channel through the bar and the reformative state of the pass between Grand Bayou and the mouth. All the old natural conditions of the pass from its head to the gulf, and far beyond the sea ends of the jetties have been disturbed by the construction of the latter and by the auxiliary works, especially those at the head of the pass which are intended to deepen the shoal at that point. When the whole work is finished and the normal condition of the pass restored by time, there will be very many interesting and valuable facts which no doubt will be given you.

A very important question and one that cannot be definitely determined by facts until the completion of the jetties, is that of an accelerated bar advance due to the construction of the jetties. All that we can state now, bearing on this question is, that although there has been a pushing outward of the upper part of the outer slope of the bar, due to three or more temporary causes; *first*, the impossibility of constructing the whole line of jetties instantaneously; *second*, the large amount of material excavated and carried out to sea; *third*, the closing of Grand Bayou and a new load of sediment given the water in addition to its already heavy burden; and *fourth*, the non-completion of sea ends of the jetties to a point designated by the Advisory Board of Engineers, who assisted Capt. Eads in determining the plans for construction;—yet careful surveys and calculations show a deepening instead of a shoaling immediately in front of the sea ends of the jetties. The water prism extending seaward from the old crest of the bar, and covering an area of 52 acres, has increased from 2 000 000 cubic yards to 2 200 000 cubic yards, or, in other words, the average deepening over the whole area investigated is $2\frac{1}{2}$ feet. The survey on which the calculations were based was made May 29th last. A survey by radial lines of soundings, extending 5 miles out from the jetties, made in May by Mr. H. L. Marindan of the U. S. Coast Survey, shows by comparison with the chart of the survey of May, 1875, also made by him, that there has been an average deepening over the whole area surveyed.

The volume of the bar removed during the first year of the work, without reference to amount moved in the channel as required by law, or, in other words, the total excavation, exceeds 3 000 000 cubic yards. The following table shows the volume of the bar moved in reference to a channel 20 feet deep and 200 feet wide at that depth.

The results are obtained by placing on the cross sections drawn on profile paper, a normal section as we find it in the pass above the jetties. This section has a centre depth of 2 feet below the 20 feet line and side slopes of 1 foot vertical to 20 horizontal. This normal section is made on tracing paper, then placed over the cross sections and the area is thus obtained for the calculation, as to the volume to be moved to make a channel of the normal section as described.

TABLE VI.

Volume in cubic Yards of the Bar removed in Reference to a 20 Feet Channel.

Date of Survey.	Volume to be Moved.	Volume Moved.
May, 1875	1 037 635	....
Dec. 25, "	622 680	414 955
Jan'y 27, 1876	524 909	97 771
" 31, "	506 643	18 266
Feb'y 17, "	499 715	6 928
March 4, "	305 547	195 168
" 25, "	256 655	48 882
April 25, "	163 619	93 036
May 29, "	22 627	140 992
June 30, "	13 977	8 650
July 31, "	4 985	8 992
Aug. 14, "	75	4 915

A part of the material has fallen over the submerged jetties during their construction, or has been driven in by the waves from beyond the sea ends of the jetties and lodged on their sea slopes. 1 000 000 cubic yards, at least, have piled themselves up against the sea side of the west jetty, an enduring and solid bulwark against the storms. The remainder has gone far to the westward, no doubt carried there by the prevailing westward coastwise current.

The so called "littoral current," is a current generally existing and has generally a westward course. Whether it is caused principally by the prevailing northeast winds or by a more constant and stronger influence, is somewhat doubtful. We have strong evidence from an examination of the general formation of the coast of Louisiana and Texas, from the eastward trend of the lower river by the excess of accumulation of sediment and consequent shore formation westward, from the general opinion of longshoremen and pilots and from our own observa-

tions, that there is a distinct and deep moving westward littoral or coastwise current, strong enough to remove far from the mouth of our new channel the sediment carried out by the river, and which will postpone for a century or two the re-formation of the bar.

In regard to action of Gulf storms upon the work, I will state that during a severe storm on March 5th, which continued several days, a few mattresses on the sea end of the west jetty were destroyed, but the work had not been consolidated nor sufficiently covered with stone. The plan of final construction provides for jetty heads of enormous base, easy slopes and a covering of heavy stone. From the experience had, we have no doubt of the ultimate stability of all parts of the work, both in reference to the Gulf storms and the river currents.

The facts given and the tables and sections exhibited are the result of careful surveys and faithful office work.* Extended and often repeated surveys have been needed to inform us of the progress of the works and the results they are constantly accomplishing, and often so rapidly that our surveys could not keep pace with them.

The paper under discussion gives us the pivotal fact, which guarantees the success of the jetties, that the deep water of the pass follows close behind the bar in its seaward march. Examine the tables presented and you will see that this fact is clearly and forcibly illustrated. It may be said, truthfully, that twelve months of work and its accompanying results have proved every theory advanced and every prophecy made in that paper. It and this account—one presented at the commencement, the other, one year after the beginning of the work—are substantially the same, the latter being simply illustrative of the former.†

Mr. Charles W. Howell.—This paper deals in a somewhat general manner with one of the most interesting, important and difficult problems in hydraulic engineering, that has ever claimed the attention of American engineers. For those who have long been engaged upon it and who have presented solution after solution, as the factor "commercial importance" has grown with the growth and necessities of the Mississippi valley, it possesses an absorbing interest that precludes the mere airing of opinion. Its ultimate solution has now become of the utmost importance to the people of the valley, and cannot be looked for with unconcern by the people of the great cities of the Atlantic seaboard, or by the great corporations which now control by their artificial highways the commerce of the West. That it is a difficult problem is evidenced by the forty

* For the accurate instrumental work and the systematic exhibition of results, as shown in the tables presented, I am largely indebted to my first assistant, Mr. Max E. Schmidt. † There was 21 feet of water through South Pass bar, August 15, 1876.

years of discussion which it has provoked, and in which many of the ablest engineers of our country have taken part.

The necessities of commerce have finally narrowed discussion on this: can the problem find its solution in a lateral canal, or in a jettied river mouth? This question has been thoroughly discussed not only by the fourteen engineers specially appointed to decide it, but also by others more or less conversant with the facts. The fourteen were equally as regards number, divided in opinion except on one important point, namely, that a "lateral canal" was practicable.

On the one hand it was claimed, in effect, that it was the highest duty of an engineer, in case of emergency, such as was presented, to first recommend that measure of relief which was certain, and afterward experiment at leisure. On the other hand, it was urged that the prospective advantages of an "open river mouth," together with a reasonable hope of obtaining it, justified recommendation of trial of the jetty system.

The question has been decided by the representatives of the people who are interested, and it is believed on other than professional opinion, viz: on the purely business proposition, "no cure, no pay." The jetties at South Pass are now in progress. Within a few years, the practical demonstration they will give will settle present difference of opinion as regards jetties and canal, and perhaps give us a chance to argue about canal routes. In the face of this, it is perhaps best for those who are satisfied with their record on the subject, to stand on that record.

Now that discussion is barred by attempt at practical demonstration, there is yet room for suggestion and inquiry. The most prominent parts of the paper are those which show the writer's reliance in precedent. It appears proper to say that precedent in works of river and harbor improvement is very often difficult to find and dangerous to handle.

The Sulina-Danube precedent has been quite fully discussed. Mr. Roberts does not offer anything new about it. It is therefore presumed that his personal inspection simply resulted in verification of facts previously known. These facts, which have been published so as to be within the reach of every engineer in the country, show so many radical points of difference between the conditions observed at the Sulina mouth and those observed at the South Pass of the Mississippi, that it does not appear surprising that many engineers should think the assumed precedent valueless, and only deceptive because of one thing—success attending jetty application at this one place. It may be that those who have based their faith upon this assumed precedent may find themselves in the position of those French engineers, who many years ago selected the jettied

drift bars of our Northern lakes as precedent for the mouth of the Rhone. The value of imitative engineering cannot be underrated, but it is often overrated in the works of the kind under discussion.

The writer calls attention to "one notable fact," which I will state in my own words. In the body of the Sulina branch, where the width has been artificially reduced to 450 feet, the depth is not so great by 3 feet, as it has been for the past ten years, on the outer bar between jetties, 600 feet apart. If this does not suggest anything else, it at least suggests this, namely: the volume of water passing between the jetties could not have been greater than that admitted between the training jetties above; consequently the velocity of current must have been greater than that between the former. There must have been something the matter with the bed of the river, similar (to sustain precedent) to the unscourable blue clay found at the mouths of the Mississippi, else according to the new jetty theory we should have the following: a width of 600 feet gives 21 feet depth, hence 450 feet width should give 28 feet depth.

Speaking of the Damietta branch of the Nile, which has a drift bar obstructing its mouth, similar to that at the Sulina mouth of the Danube, the opinion is expressed that this would be the place for a canal, should commerce warrant the improvement of this branch, but the jetties would not be applicable. Now, that we are collecting assertions and opinions rather than facts, it would be interesting to know fully on what grounds this opinion is based. Might not the jetties be made to arrest the drifting lands which now choke the river's mouth, at least as effectually as do those which protect the harbor of Suez, not far distant? Might not the jettied entrance of a canal be blocked up quite as quickly by these sands as a jettied "open river mouth?" Can the advantages of a "great open mouth" be overlooked should commerce demand improvement? The single fact is stated, that during low water in the Nile, the water of the Mediterranean backs up some 10 miles to the city of Damietta. In the Mississippi, it has been frequently observed during low water, that at New Orleans, 110 miles from the mouth of the river, the surface of the river water has been below the surface of the water in Lake Pontchartrain, which is directly connected with the Gulf. On the opinion referred to, may we not hereafter make the Nile a precedent for the Mississippi, when we give up jetties and come to a canal?

Mr. John G. Barnard.—Some time ago, I declined to take part in the discussion of this paper at this time, saying that such consideration of the subject had been nearly or quite exhausted, especially as the work which was the best exponent of the matter, was actually in progress. The

history and theory of the undertaking have been so thoroughly gone over that there is little for any one to add. I feel confident, however, that Mr. Corthell's lucid statement of the process of channel formation, as the jetties progress, must be gratifying to the advocates of an open river mouth.

The matter having to-day been so fully brought before the Society, I will briefly enlarge upon the history of this improvement, as, from the beginning of the several projects, I have been familiar with them.

I was in New Orleans when the first survey of the delta was made by Talcott. The Chief Engineer of the State of Louisiana, Benjamin Buisson, had conceived, in 1832, the prospect of a ship canal, which was to occupy the site identically of one projected three years ago. Six years later, Major W. H. Chase, U. S. Engineer, adopting the idea, presented with some detail, though without drawings, a project for a ship canal of the same location. I was familiar with all these projects from their origin, and have had occasion to execute works of various kinds by which to arrive at a knowledge of the character of the soil and to make excavations in it, and in particular, at Fort St. Philip, near where the canal was to be. The canal project was revived by a resolution of Congress in 1871, which simply called for a survey and plans for a ship canal. It was committed to an officer of U. S. Engineers, who, in 1873, submitted a project which was afterwards laid before the Board of which I was a member.

The question as to the practical working of a canal in this locality became more and more involved in doubt in my mind, as I studied this project. The inadequacy, to say the least, of a canal as an outlet for a great and important port such as that of New York, were the Narrows and East River shut up and a ship canal substituted, would be apparent to every one. Moreover in the case before us, there are doubts as to the practical working (successfully) of a canal when made, and all its enormous expense incurred, peculiar to a Mississippi river ship canal.

I became more and more convinced, as I studied into the matter, that an open river mouth was what was required, and my position was to have further investigation made, before the Government should be committed to the costly and doubtful ship canal. Congress ultimately adopted this course; and ordered another Board of Engineers, composed of three army, three civil,* and one Coast Survey engineer, which resulted in the selection of the jetty system.

* Mr. W. Milnor Roberts, the writer of the paper under discussion, was one of them.

In all the discussions about the matter, I am safe, I think, in saying there were but two arguments used against the construction of jetties. I, while giving such arguments due weight, claimed that the question was not purely an engineering problem, maintaining that the importance of the result aimed at demanded that the trial should be made, and if there were really fair engineering grounds for thinking it would prove successful, the circumstances absolutely demanded a trial. And while always admitting the *practicability* of canal construction, I have always, too, maintained that besides its obvious *inadequacy*, there was, peculiar to this Mississippi work, a doubt as to the practical *working*, quite equal to any doubt as to success of the much less costly jetties.

One argument against the jetties was, that you could not build them; that the ground was too soft. All engineers are familiar with the hindrances to engineering progress which have arisen from assuming the facts. For instance, we know how the *assumption* that friction alone could not be relied on for locomotive traction on railroads, retarded the invention of the locomotive and the development of railway traffic.

I had studied carefully the jetty contract made by the Government twenty years ago with Craig and Rightor, which, though abortive, proved that the lateral shoals on which the jetties were laid, were by no means so soft as they were assumed to be, by those who maintained this argument. Col. Long, U. S. Engineers, reported to the Government that the bottom was not soft, but sandy, or sand mixed with clay; indeed a large number of the piles driven there twenty years ago are standing yet.

An important point, and one that seemed quite serious, was as to whether the bar would advance with increased rapidity. The South Pass bar has hitherto advanced annually about 130 feet. But the argument was, as put forth, that it would advance much faster than before. I never admitted that there could be an accelerated local advance; as the sedimentary matter is distributed over a very wide surface, the rate of growth of the bar would not be materially increased; the predicted accelerated advance was a matter too purely theoretical to be made an obstacle to a work so urgently demanded. There is one thing, however, which is indisputable. The rate is greater or less as the quantity of sedimentary matter discharged through the outlet is greater or less. While the process of jetty building is going on, a great and abnormal increase of this matter is caused by the scour superinduced upon the bar itself; hence a temporary abnormal advance might be looked for, and which, if observed, would afford no argument for rapid bar advance. The engineers of the last Board who adopted the jetty system, assumed and admitted and made

their estimates and plans upon the fact that there would be an advance. I make mention of this with some emphasis, conscious that some of the jetty advocates contend that the bar advance will be checked or cease with the application of jetties. My own opinion is, that the advance would rather be checked than accelerated ; but this, like the other theory is too purely a matter of speculation to be made a basis of engineering projects. While firmly believing in the jetty system as a means of having an open river mouth to the Mississippi, there are incidental questions to which time alone can give the solution. The observations of a day or a month require confirmation by those of a year or even of several years.

Mr. W. Milnor Roberts.—Since I presented, in October, 1875, the paper now being discussed, work upon the jetties has been steadily prosecuted under the direction of Capt. Eads, whose agreement with the Government requires him to increase the depth across the South Pass bar to 20 feet before receiving the first payment. A depth of 17 feet has been* secured entirely across the bar, where one year ago, before he began work, the depth was only from 7 to 8 feet ; while the 30 feet river depth has been gradually advancing outward. The distance across the bar between the river contour of 20 feet and the ocean contour of 30 feet, which one year ago was over 10 000 feet is now reduced to about 390 feet. The work on the jetties being so far advanced that the abrading power of the river is continually cutting away the top of the bar between the jetties and increasing the depth of the navigable channel across it. The jetties have thus really created a channel where none existed, and recently several of the finest ocean steamers trading to New Orleans have made use of the South Pass, both going in and coming out. The plan pursued by Capt. Eads has been to depend wholly upon the natural force of the river current, now mainly confined between the jetties or piers, without employing either scrapers or dredgers, the use of which would have facilitated the deepening of the channel.

A large quantity of material, consisting of sand and clayey mud, has thus been cut away by the river current and swept seaward. Contrary to my anticipations, very little of this material has settled immediately in front, where the depth of water inside of the extreme outer end of the jetties, instead of decreasing has been increased since the works were begun.

The process appears to have been, the carrying directly seaward all, or a very large proportion of the river sediment held in suspension while the heavier particles appear to have been distributed laterally by the

* June 15th, 1876.

movement of the ocean water in front; easterly winds moving the material at the bottom westward, and westerly winds eastward.

Prior to the beginning of the work, during the discussions which took place in the fall of 1874, I believed (and so argued) that during the process of removing nearly, or perhaps quite, 3 000 000 cubic yards of material from the old bar by the river current, aided artificially by dredging and scraping (in order to save time), the outer face of the bar would be considerably extended seaward, thus calling for a greater length of jetties than might at first view appear necessary; and, on account of this assumed gradual extension of the outer slope, I advocated the postponing of any permanent work at the ends, analogous to a bulkhead, leaving the extremities of the jetties on a slope, so that the jetty work could be easily extended, as needed, without extra cost.* In estimating the cost of the works, the Commission allowed for an assumed advance, after the completion of the jetties in the first instance, at the rate of 130 feet per annum. Differences of opinion, then as now, existed among different engineers, both in the Commission and out of it, respecting the probable rate of advance that would in the future take place. That is to say, on such an advance of the bar as would require the extension of the jetties in order to maintain the depth which should be originally secured by them. This is a question which only time can completely determine. It will, of course, be settled by the actual movement and ultimate depositing of the sediment, which will be annually delivered through the South Pass outlet, modified more or less, by the movement along the delta of the sediment brought down by the other passes and discharged outside on the advancing semi-circular frontage of the delta. It is important to consider the shape of the delta.

The special point I desire to make here is this, that thus far the filling up in front, caused by the carrying seaward so much of the old material of the bar, has not been nearly so great as I anticipated it would be. The action outside seems to be somewhat analogous to that which has occurred at the Sulina mouth of the Danube, where the littoral current running along the coast from the north, sweeps the sediment southward, while the river maintains a navigable channel depth through it, of 21 feet, where originally there was only a depth of 8 or 9 feet.

The length of the jetties at this mouth of the Mississippi is nearly 2¼ miles, while those at the Sulina mouth of the Danube extend out less than one mile. Hence, with an easterly wind piling the water up against

* I believed then, and I still believe, that all that will be needed at the extremities of the jetties will be a flat sloping termination, not projecting above the surface of the sea, composed of the same material as the residue of the jetties.

the east jetty, and driving it away westward from the west jetty, the cutting action outside of the jetties (with a wind of equal force and continuance) should be greater at the Mississippi mouth than at the mouth of the Danube—other things being equal. The two places are not, however, exactly similarly situated; but it is the opinion of Sir Charles A. Hartley, the engineer who constructed the works at the Sulina mouth of the Danube, and who has carefully examined the South Pass, that the latter offers a better chance for maintaining a deep sea entrance to the river than did the Sulina mouth.

Most of the engineers who have examined and carefully studied the natural action which has been going on for ages, at the mouth of the Mississippi, and who favored its improvement by canal in preference to jetties, did not claim that the river would not cut a deep channel between the piers; but that the bar would immediately re-form outside, requiring an immediate and constant extension of the jetties, while some contended that the bar itself was so soft and so treacherous, that it would not sustain the necessary structures for confining the flow of the river between them. The strong and reiterated assertion to this effect, insisted upon publicly and otherwise, had very great influence in my own mind against the jetty plan for the mouth of the Mississippi, until a very careful personal investigation of this most essential point satisfied me thoroughly that it was entirely erroneous. From that moment, I could no longer entertain a doubt of the ultimate success of the jetty system for the Mississippi mouth, a system which had proved so successful elsewhere.

I am not aware that any engineer at this time claims that jetties cannot be built and maintained at the bar of the South Pass; I believe that the only or the chief argument now made against their feasibility, and advisability, by those engineers who have thoroughly investigated the place and its advantages and disadvantages, is, that according to their view, the probable cost of extending them annually will be excessive, on account of the rapidity with which the bar will, as they believe, re-form in front, demanding, as a consequence, their extension correspondingly. This, in the nature of the case, is a matter upon which engineers may for a while continue to differ; especially as it is almost if not quite impossible to apply formulæ, which can be relied upon, to determine precisely what the future will show. The Mississippi river carries annually an immense quantity of material which is deposited by its several passes in the Gulf of Mexico; but the Gulf of Mexico is a vast receptacle covering more than 500 000 square miles, and presenting a descending bottom in front of the Mississippi delta, 200 feet deep, at

the distance of 2 miles, and which deepens more rapidly farther out, so that it attains a depth at the distance of 50 miles, of over 10 000 feet.

Since this immense gulf is so much deeper in front, and since the fan shape position of the mouths is constantly expanding, widening out the frontage of the deposit, it is reasonable to assume that the rate of advance of the delta, gulfward, must be gradually, but constantly diminishing. When we find an old Spanish magazine, built of brick, at least 150 years ago, still standing without a crack in its walls, 5 feet above the water level, within a few miles of the extreme outer end of the present delta, it affords additional reason for the belief, that during the next hundred years the jetties at the South Pass need not be extended beyond the allowance made by the Commission of Engineers, which decided upon the plan, and selected this pass for the construction. That allowance being 130 feet a year, would, in one hundred years, amount to 13 000 feet, nearly 2½ miles. There is no reliable evidence tending to show that the whole delta has in the past hundred years moved out so much as 2½ miles; and if it had moved out that far, only, in a hundred years, it is mathematically certain, from the premises given, that it will move out less than that distance in the next hundred years.

The cutting of a deep channel through the bar at the head of the passes, between the main river and the South Pass, and the cutting of a deep channel through the outer bar to the Gulf, may somewhat increase the flow through the South Pass, and consequently cause it to discharge more sediment, at its mouth (and which will also be augmented by the closing of the Bayou Grande), yet this additional quantity, when carried out and distributed in such a very different manner, from what it has been distributed in the past, may prove to be but an inconsiderable element of the problem.

If it be conceded, that in the lowest stage of the river, water enough flows through the South Pass to maintain an adequate ship channel between the jetties, the only remaining practical question to be discussed, is the future distribution in the Gulf, outside of the jetties, of the sedimentary discharge. It is argued by some engineers, and by others, that immediately beyond the outer extremity of the jetties, which confine the water within a limited width of channel, 1 000 feet, the river water, when released from lateral confinement, will at once spread in all directions, lose its velocity, and consequently let down its sediment, not only in close front, but near by on both sides of the channel entrance.

While this may be measurably true, to a small extent, it is met by the fact that the bulk of the fresh water flow (which of course remains at the

top, above the heavier salt sea water) does not immediately lose its velocity, but passes directly onward for a long distance gulfward, depositing much of its suspended sediment far beyond the jetties, in deep water. I have myself seen the discolored fresh water of the river Amazon on the surface of the ocean, nearly 150 miles from its mouth.

Several irregularly acting causes operate to control and determine the final disposition of the sedimentary material discharged from the South Pass between the jetties. These are:

I. The irregular quantity of water delivered by the Mississippi river at different seasons of the year; the quantity during heavy freshets being three times as great as it is in extreme low water stages; and the irregular and very different proportions, at different periods, of the quantity and kind of sediment brought down.

II. The tidal action (which here takes place only once in twenty-four hours) ranging usually from 1 foot to 2 feet in height.

III. The action of the winds. Sometimes the tidal and wind action are in direct opposition, sometimes in conjunction, and sometimes partially one way, or the other way. Sometimes the river current action is in conjunction with wind and tidal action; sometimes in opposition to both, or one, as the case may be; and in proportion to the relative strength of these three different forces, and the relative quantity of moving sediment in suspension and on the bottom, must be the resultant action outside of the jetties; and to some extent, at times, between the jetties, there may be temporary partial deposits of silt.

It may be possible, by assuming given quantities of water, and given proportions and weight of sediment carried in suspension, given rates and depth, of natural flow of the fresh water, given heights of tide and given force and direction of wind, to make up a complicated theoretical formula, showing approximately the probable action of the water outside of the jetties under certain assumed conditions, but it would have no practical value whatever, as it would relate only to one special set of assumptions, which might not occur together once in any year, if ever.

Instead of any such formula, the investigating engineer must be content, at first, with mere general statements of the causes operating, and with the facts to be obtained hereafter from careful soundings. Future soundings, compared with those of the past, will surely show what becomes of the sedimentary material carried into the gulf through the South Pass. Experience tells us, that where a considerable volume of river water flowing rapidly, enters a salt water ocean (or gulf), it does not immediately lose its identity as a river stream, and that, in the main, it

continues to flow directly onward over the sea water, for a long distance seaward, and only very gradually loses its velocity. Meanwhile, it must be granted, that portions of the fresh water—being a trifle higher than the sea level at the outer ends of the jetties—will naturally spread on each side, and flow away laterally; but this quantity, near to the jetties, where the onward velocity is greatest, must be small compared with the principal flow.*

In long continued heavy blows from the eastward, a sea current, crossing this river discharge nearly at right angles, must tend to bend it toward the west, and, upon the subsidence of the gale, and especially when the wind hauls around and blows from the westward, a strong return current must occur in front of the South Pass entrance. Such action and reaction have the effect of distributing a portion of the sedimentary matter in suspension westward and eastward of the direct line of flow, and also of moving along the sea-slope a sand and mud previously deposited outside. Thus, although the bar in front of each of the mouths of the Mississippi has been projected farther into the Gulf than the bays which are left between them, yet these bays are also constantly shoaling, and gradually following the general movement of the delta, gulfward. But for these actions and reactions created by the tide and winds, in connection with the variable river discharge, each pass would naturally construct for itself a long, comparatively narrow embankment, leaving deep water between the mouths. An examination of the charts of the coast survey will show, very clearly, the result of the several operating causes referred to.

It is not, therefore, surprising that there should be differences of opinion among engineers respecting the future action in front of the jetties now in the course of construction; but if experience, and judgment based upon it, are of value, there would seem to be no substantial reason for doubting ultimate success in creating and maintaining a much better ship channel than has ever been obtained at any mouth of the Mississippi, a channel which can be kept always navigable for deep ocean vessels at a very moderate annual cost; the cost being absolutely insignificant, when put into comparison with the great national benefit of a deep channel entrance from the Gulf to the deep water of the river.†

* We must bear in mind, that the regimen of the discharge is totally revolutionized by confining the water, in its deepened channel between the jetties, instead of allowing it to spread out in every direction as a shallow stream, losing a large portion of its velocity by friction against the bar. The concentrated volume of fresh water will exert a power which has never before been seen at that Pass.

† Many engineers who have not had occasion to make a critical investigation of the characteristics of the Gulf of Mexico, may be somewhat surprised upon reflecting, that on a line

In fairness to those engineers who have advocated the jetty system in preference to a canal, for the permanent improvement of the mouth of the Mississippi, it may be proper to mention a few points which have not been sufficiently dwelt upon, in any of the discussions which have publicly appeared.

There has been a pretty general, if not an almost universal, impression left in the public mind that a canal, giving deep navigation between the river and the Gulf, would not involve the construction and maintenance of jetties. This is very far from being the case. At every point which has been proposed by the friends of the canal plan, jetties were a necessity in order to reach the deep water of the Gulf, and should any other point be selected, it would be necessary to construct and maintain jetties through the sloping beach to the deep water. These canal end jetties would differ from the jetties now in course of construction, chiefly in this: that no strong river current carrying sediment would pass through them. There would be nothing more than the lockage water discharged by the lockage of each vessel entering and departing. Nevertheless, the canal jetties would be exposed to the action of the sea, from the tides and winds, and the mouth of the canal would be liable to silt up from two causes, namely: from the sediment which would be carried in suspension by the lockage of the river water, and from the shifting of the sands and mud by sea action in front. The canal outlet referred to, would, in the first instance, have to be made, by dredging to secure the required depth, and at the points where a canal has been proposed, the depth of water outside, on a nearly level sea plateau, is only about the same as the artificial canal depth proposed, say 30 feet, or 5 fathoms. And if the entire delta is moving forward into the Gulf, which all engineers assume is the fact, it could only be a question of time when the plateau in front of the canal debouchure should be filled with deposit of sand and mud. Meanwhile, the canal jetties would be exposed to the easterly and southeasterly storms of the Gulf.

According to my judgment, as fully explained and elaborated before the mixed Commission of Engineers, after we had carefully examined the whole subject and all of the points on the ground, a canal—if a canal plan were to be chosen—should be made to debouche not at or near Fort St. Phillip or any place so far up the river, but near the river

projected forward from the end of the Mississippi delta to the nearest land in front—which is the Island of Cuba—it is nearly 600 miles, or as far as it is in a direct line from St. Louis to New Orleans. If we knew how many centuries the Mississippi has taken to push the delta from New Orleans to the present land end, we could form a better idea of what it may do in the future.

mouth ; so that the canal jetties would end on a beach sloping to the deep water of the Gulf, and where, instead of having 5 to 6 feet height of lockage, there would be nothing more than the tidal lockage of 1 to 2 feet. This would constitute a much better canal entrance than any that has been indicated by the friends of the canal system. The principal objection to going so near the mouth appeared to be that the material there was too soft, as compared with the material nearly opposite Fort St. Phillip on the Gulf ; which, according to our examination, is not the fact ; the material is essentially the same, and would admit of the safe construction of locks at either site.

If to-day, the lock and canal outlet jetties, and the open river mouth jetties were, respectively, completed, no engineer would be warranted in claiming that the canal outlet could be maintained intact and of full depth, at less annual cost than the open river mouth can be maintained.

The canal outlet would of itself be inert, yet still exposed to the sea action, tending to silt up in front and in the outlet itself ; while the river outlet is vigorously belligerent, having in itself a powerful force in its natural river flow, to drive out the sea and thus to maintain its depth between the jetties.

Since this discussion began, it has occurred to me that a few words in regard to some points touched upon by Maj. Howell may serve to further elucidate the subject. With the bulk of his remarks containing general views, I believe there is now no material difference of opinion among the advocates respectively of the canal and jetty projects. The real difference between these two parties is, that the friends of the canal project believe that the jetties will be a failure, while the friends of the jetty system believe that they will be a success, and a short time must settle this question. At all events, an attempt to solve the problem practically is now going on—not at the expense of the Government, but of Capt. Eads and his friends, who have put money, labor and brains in the undertaking.

Some of the points made which need a little correction, seem to have been due to incomplete information, or want of time thoroughly to study the cases to which they refer. This is especially so, in regard to the comparison between the Damietta branch of the Nile and the South Pass of the Mississippi. These are radically different, not only in the shape and nature of the bar, but in the very important particular, that while there is always a large amount of water flowing out at the South Pass, even in the lowest stage of the river, scarcely any at all flows out in the lowest stage of the Damietta branch of the Danube ; and within

the memory of man there has never been anything like a ship channel across the bar, even when the Nile was in its highest flood—as it was, when we visited the place in 1874; it was then higher than it had been for sixty years.

An artificial harbor could be constructed and kept clear at some other point away from the front of this excessively fluctuating river, but not opposite to it, or by allowing the river sediment to discharge into it during the floods. This is precisely what I meant, when pronouncing this outlet of the Nile a proper place for a *canal*, which should have its debouchure some distance away from the discharge and immediate influence of the river.

It is the absolute cessation, for a considerable period every year, of the river flow to the sea, that creates the peculiar characteristic of the mouth of the Damietta branch. It is not in the least analogous to the case of the Mississippi river at New Orleans; for there there is always, at all seasons, the whole immense flow of the Mississippi river passing, and the sea water by way of the river, never comes within a hundred miles of New Orleans.

On the other hand, in the low water stage of the Damietta branch of the Nile, the river thence to the Mediterranean becomes nothing more than an arm of the sea, and the waves play with the east bar in front, precisely as if the river were obliterated, which for the time being, and until the annual rise from the upper Nile comes, it is. For more than a thousand miles above its mouth, the Nile does not receive a single tributary, large or small, nor is it ever augmented by rains on this distance. All its waters come from the vicinity of the equator, about 30° farther south. The Nile is a river *sui generis*, which must be studied by itself; hence, works which may be appropriate at the mouth of the Mississippi, would be worse than useless at the Damietta mouth of the Nile, and this is a point which Maj. Howell does not seem to have considered.

ON HYDRAULIC EXPERIMENTS WITH LARGE APERTURES OF DISCHARGE.*

Mr. Theodore G. Ellis.—I will refer to a criticism of this paper in the "Engineering and Mining Journal;"† the writer does not seem to have given as much attention to the subject as I could wish in order to

*Referring to—CXVIII, Description and Results of hydraulic Experiments with large Apertures, at Holyoke, Mass., T. G. Ellis, page 19. †Engineering and Mining Journal, Vol. XXI, page 467.

understand exactly what was meant. He appears to think it was intended to assume a new value for the force of gravity because it was computed from La Place's formula with Bessel's constants for the latitude of the place and the height above the level of the sea. This was done to obtain the exact value of the force at the place, and not to vary from the ordinarily accepted value of the force of gravity. I wished to notice even the smallest fraction of error possible, and that is why this quantity for the place was computed, instead of taking the usual approximate number.

Another point mentioned is, that there is some fractional discrepancy in the value of the co-efficients given, so that it is better to take a mean of all the experiments as a mean co-efficient for all the apertures. The formulas which we have used up to the present time have been chiefly derived from the experiments of Poncelet and Lebros. The extreme head under which these experiments were tried was erroneously stated in the paper to have been about 10 feet, when in reality they were only taken to a height of 1.7 metres, or about 5½ feet; their tables were extended upward and downward to include about 10 feet, so, as copied into other works, they are generally taken up to that distance. It was not until I examined the original experiments that I discovered the facts. Their actual experiments show that for every different size and shape of aperture there is a different curve for the value of the co-efficients, and after getting above a certain head they all approximate a minimum value that is apparently in the neighborhood of about 60; but as the head diminishes, the value of the co-efficient increases, so there is also a maximum point in the curve, and this maximum is greater with the smaller apertures. I have lately been engaged in examining carefully these and other experiments, in connection with my own, to discover the law of these curves, and their relation to the different sized apertures.

There are some discrepant observations recorded in the experiments, but it was not considered best to discard them. The practice of not recording apparently discrepant observations is a bad one; often what from preconceived opinion was supposed to be an error, has proved to be the key to the whole law.

I intend to carry this subject further, and am at present examining all the best known experiments, with a view to make a table of co-efficients for the use of myself and others.

AMERICAN SOCIETY OF CIVIL ENGINEERS.

INCORPORATED 1852.

TRANSACTIONS.

NOTE.—This Society is not responsible, as a body, for the facts and opinions advanced in any of its publications.

DISCUSSIONS OF SUBJECTS PRESENTED AT THE EIGHTH ANNUAL CONVENTION.*

ON LEVEES.†

MR. CALEB G. FORSHEY.—The paper under discussion has much merit; especially in the history of this phase of hydrologic science. The "Mississippi levees" have been treated with more or less detail by all writers on the history or physics of this great river: Pitman, Stoddart, Martin, Flint, Monette, Forshey,‡ Thomassy, by Humphreys and Abbot, and now by Bayley. It may be considered as well recorded in the pages of hydrographic literature. Referring to the paper, attention is first called to—

EFFECT OF LEVEES UPON THE FLOOD LINE.—The writer undoubtedly places that question beyond cavil, if anything was before needed. Prior to levees, every bend gave an outlet of 3 or 4 feet over the banks, for 2 or 3 miles of current. The sum total of this outlet capacity was much greater than any crevasses that in recent times inundated the country. "There are many banks on the river front where the natural surface of the ground has never been overflowed in the memory of man." This is termed a fallacy by the learned authors of "Physics and Hydraulics of the Mississippi river," chiefly because "there have been crevasses more or less, every flood." I respectfully dissent, and allege that if the waters before levees existed were high enough to deposit these banks, they were higher than at present under the influence of levees.

*Continued from page 298. †Referring to—CXXI. Levees as a System for Reclaiming low Lands, G. W. R. Bayley; page 115. ‡ Delta of the Mississippi, and Physics of the River, Control of its Floods and Redemption of the Alluvion, a paper read before the American Association for the Advance of Science, 1872. The Levees of the Mississippi River, Transactions, Vol. III, page 267.

The levees were built from below; and the gradual manner of their application enabled the river's bed to adapt itself to the new servitudes. We are forced to the conclusion that the river's bed has greatly enlarged. In addition to the sections measured by me, as stated,* I have since assisted Lieut. Davis in a very careful remeasurement of Sections 5 and 6 of the prime base of the Delta survey, as measured by me in 1851 for Gen. Humphreys, chief of that survey. The remeasurement of these sections shows a total enlargement of 13 646 square feet of the sections sounded by the Delta survey above the prime base, and within 2 miles above Carrolton I have resounded 4 sections, and Mr. W. H. Williams has re-computed them with care and precision.

They correspond to sections of Delta survey, thus:

	Square Feet.		
Section.	1851.	1872.	Difference.
56	226 267	233 193	+ 6 926
58	227 458	226 877	— 581
69	209 241	229 325	+ 20 084
78	189 128	194 945	+ 5 817

These illustrate the allegation of an increase of channel capacity.

The facts, of the shifting banks of the river and the levees preventing the deposit of materials upon the ground formerly visited by the river, are of some importance to record, while they are remembered; the localities where this testimony is furnished proving the early floods as great as more recent ones. I would, therefore, refer to a series of points not changed, where the water has never been *a foot* above the banks since the application of levees. This kind of testimony is now rare, and the persons who remember the localities are much rarer.

Beginning below New Orleans and ascending, they are:

1. The upper extreme of Point La Hache, 40 miles below. L.†
2. Deer Range, above landing, 36 miles below. R.
3. Concord plantation, upper end, 25 miles below. L.
4. Fort St. Leon, 17 miles below. R.
5. Becka plantation, 13 miles below. R.
6. Belleville Foundry, opposite lower portion of the city. R.
7. Friendship, Labarre's place, 2 miles above Carlton. L.
8. Union, Dussieux plantation, at lower end, 13 miles above. R.
9. Red Church, 26 miles above. L.
10. Bonnet Carre Point, Glendale, Ferry landing, R.

* Page 137. † L for left, and R for right bank.

11. College Point, 60 miles above. L.
12. Australia, lower line, 115 miles above. R.
13. Glennons, 160 miles above. R.
14. Bayou Sara, below opposite 168. R.
15. Home Place, 235 miles above New Orleans, 30 above Red river. R.
16. Ellis Cliffs, below opposite, 252 miles above. R.
17. Goodman's plantation, 310 miles above New Orleans, 3 miles above Waterproof. R.
18. Perkins, above mouth of Vidal Bayou, 360 miles above. R.
19. Wilkinson's Point (T 15, R XIV lower side). R.
20. Hendersons, lower part, T 19, R XIV, 412 miles above. R.
21. Pilcher's Point, upper end, Bunch's Bend, 460 miles above. R.

At each of these points, there was in 1872, testimony of adequate kind, that the highest flood mark was not higher now than when the land behind the levee was deposited. The levee was small, less than 2 feet high, in many cases not 1 foot, and the flood mark often less than 6 inches. The water depositing the land must have been 1 foot deep.

TENDENCY OF SOUTHWARD FLOWING WATERS TO IMPINGE AGAINST THE WEST BANK.—Reclus is well sustained in his remark upon the Mississippi as not confirming "the law of displacement of running waters." I however, dissent from the doctrine as alleged by him and affirmed by Mr. Bayley, although it appears to be sustained by illustration. The laws of physics forbid it. The earth in its rotation revolves as a whole, water and all; and there is no appreciable tendency of water rather than of solids, to incline to the West. The rate of velocity, 3 miles per hour against 900, would be inappreciable in the revolution of the earth.* The weight of evidence is on the other side.

The Mississippi river hugs the bluffs of the eastern bank from Cape Girardeau to Lat. 35°, below Memphis, a distance of 300 miles. From thence to Vicksburg, it bisects the alluvial area 380 miles, inclining to neither side. From Vicksburg to Baton Rouge, a distance of 250 miles, it hugs the eastern bluffs again, and thence to the mouths it inclines to the South East for 240 miles. In its whole distance of 1200 miles it touches but once the western bluffs at Helena, when as free as air to choose its course. And when the mouth is reached, and it divides in three, and ultimately into about seventeen mouths, it sends one-third to the South Pass and two-thirds to the other directions, as moved above, southeastward. And again; the deposit of sediment on the west side,

* I take exception to Reclus' doctrine of the westward tendency of rivers emptying southward; and chiefly because of the disproportion between the velocities. I quote the velocity of the Mississippi river at 3 miles per hour; this is the rate of channel movement, and not the movement in latitude, which is much less. The movement southward, of the Mississippi's water is as one to two compared with the channel movement. The rate compared with the movement of the earth upon its axis is then as 1.5 miles to 900 miles, and is, therefore, for a stronger reason than the one assigned, inappreciable.

thus fending off the river, is in proof that all rivers with alluvion should be driven to the eastward rather than toward the western shore.

The Mississippi river in its whole alluvial length is in singular defiance of this hypothesis. An examination of the chief rivers of the American continent, where they are larger than elsewhere, will be found to contravene the allegation of any appreciable influence. The Mississippi, Pearl, Pascagoula, Mobile, Apalachicola, Savannah, Santee, Cape Fear, Neuse, Roanoke, Pamlico, Chowan, James, Potomac, Delaware, Susquehanna, Hudson, Housatonic, Connecticut, Androscoggin, Penobscot, St. Croix, Sabine, Trinity, Brasos and Corao, with a singular uniformity contradict the assertion of westward tendency, leaving only, some small unimportant rivers on the whole North American continent apparently sustaining the hypothesis.

In South America, the direction of flow of the Orinoco, the Amazon and all smaller rivers to St. Roque, is coincident with the alleged influence, and nothing can be inferred from them. Thence all the rivers down to the La Plata run normal to the ocean beach, and do not sustain the hypothesis.

The rivers of Western Europe running south are all small, except the Rhone, Guadiana, Guadalquiver and Ebro, which affords a striking contradiction. In Eastern Europe, the Dneiper and the Don, the Volga and the Ural are all wanting in testimony in its favor.

The only rivers of any magnitude in Asia, that flow southwardly, are the Euphrates, the Indus and the Ganges, and we look in vain to these for testimony that their waters have a westward tendency in their flow.

The hypothesis would require that all rivers flowing southward, in their approach to the mouth, should hug the uplands till they reach the alluvion and then have a westward flexure in their discharge; whereas the cumulative evidence is indefinitely great that no such influence is appreciable. I have been thus elaborate, because of the high authority of Reclus, sustained by Mr. Bayley, and because of the necessity of being right in the establishment the physics and projecting the works, of a river, to which the attention of the engineering world is now drawn.

CUT-OFFS.—The cut-offs of the Mississippi have been too numerous, since its navigation by steamboats and occupation by man, to permit the return to a previous regimen, from natural causes. The assumption that it will maintain the same length, in any long period, is too violent, in recent times, whatever may have been the truth prior to the settlement of its hydrographic basin. The number of cut-offs by artificial means has

outstripped the tendency to lengthen itself, and the plane of discharge has been greatly increased in the present century. Every cut-off that has occurred has been largely assisted by artificial means. No less than seven have taken place below the mouth of Arkansas, within a half century. Probably not more that two or three of these would have occurred without aid. They have abridged the length of the river by about 106 miles, and it has lengthened itself less than 20 miles, by increased caving in the concave bends; all of which have travelled downward. The 618 miles as now measured, should be 724 miles or 17 per cent. more; the velocity of 3 miles per hour has increased to 3.4 miles; and the abrasions have been in proportion. The confinement of thé water to the channel by levees, has increased the servitude without sensibly increasing the velocity. They have assisted to increase the capacity of the channel by widening and deepening, as shown by Mr. Bayley. The cut-offs, however, have increased the steepness of the plane of discharge, and hence the ravages upon the banks. They are most disastrous in their effects, and should be prevented by all the means that law and watchfulness can devise.*

OUTLETS OR WASTE WEIRS.—The effects of these are well elaborated by Mr. Bayley, in his proofs that a bar will form almost immediately across the stream, directly below the weir or outlet. This is amply shown in my report upon Bonnet Carré in 1850.† Soundings in the fall of 1850, showed section below to be contracted 75 613 square feet as compared with section at upper end of crevasse. Soundings made during high water of 1851, the crevasse having been closed, proved that the great bar thrown across the river channel by weakening its transporting forces, in the discharge of water through the crevasse, was entirely carried away again, when the river was confined by the new levee. The difference was 23 000 feet in section.‡

This paper of Mr. Bayley, is full of suggestion and a valuable contribution to the annals of the Society.§

* I herewith submit a paper on "Cut-Offs on the Mississippi River, their Effects on the Channel above and below;" wherein the subject is discussed more at length. It will be found mainly in harmony with the paper under discussion.

† Published in Public Documents, Louisiana Senate, 1851.

‡ As I was first to announce this fact in the physics of the river, and it has given rise to much discussion since, I deem it worth re-claiming, as a great practical principle in the treatment of the river.

§ Although the writer has given frequent references to authorities, one omission is to be noted. The statistics in the remarks on Tones Bayou are furnished by a survey of Gen. Jeff. Thompson and myself, one of the most rapid and exposed expeditions, and most fruitful in the number and value of results, to be found in American engineering; see Report of Commission of Levee Engineers, January 1st, 1873.

Mr. Gouverneur K. Warren.*—My ideas on this subject, in relation to the Mississippi, are set forth in the report of the Commission on the reclamation of the alluvial lands of the Mississippi,† of which Commission I was appointed President. This Commission was composed of five members appointed by the President of the United States, under an Act of Congress, which provided that two of them should be "civil engineers eminent in their profession," and three of them. officers of the Corps of Engineers. After a thorough consideration of the subject, with the most recent data carefully collected and studied, that Commission made an unanimous report. One of the members, Gen. Abbot, of the Engineer Corps, had shared with Gen. Humphreys, the laborious investigations in regard to the subject of levees on the Mississippi river, and also the honor and credit which their contribution, known as the "Physics and Hydraulics of the Mississippi River," has received.

In transmitting the report of the Commission through the Chief of Engineers, Gen. Humphreys, to the President, a common sense of justice caused me to say: "The foundation of the report of the Commission rests upon your invaluable surveys and investigations, which, begun in 1850 and continued until 1861, are published in the great work 'The Physics and Hydraulics of the Mississippi River, and upon the protection of the alluvial region against overflow,' &c., and upon the further contributions to these subjects contained in your published official reports in 1866 and 1869." I continued; "the Commission has obtained additional data upon subsequent floods and the results of the more recent experience in building and re-building levees, as far as they are attainable, so that their report is in a great measure exhaustive of the subject, and the conclusions reached may be considered entitled to confidence."

I wish it to be known that this letter of transmittal was written and signed by myself only, just as the printed report shows, and this expression of confidence was but an individual opinion. The report, signed by all the members, gives the foundation for that confidence, and is expressed in facts and reasons. To the report itself, I therefore invite attention of those who wish to become informed of my views on the subject of levees.‡

* Presented June 15th, 1876.

† Report of the Commission of Engineers appointed to investigate and report a permanent Plan for the Reclamation of the alluvial Basin of the Mississippi River subject to Inundation. Washington. 1875.

‡ I will endeavor to furnish a copy, to every one who will apply to me for it, giving his address.

To show what these conclusions were, I will here quote them.

1°. *Cut-offs.*—"So far from artificially aiding in their recurrence, it is therefore the emphatic opinion of this Commission, that in every case they should be prevented, or at least retarded, if this can be done at any reasonable cost."

2°. *Diversion of Tributaries.*—"No such works are practicable except at enormous expense, and the injury to navigation which would be sure to result, would in any event forbid their execution."

4°. *Outlets.*—"This Commission is forced unwillingly to the conclusion that no assistance in reclaiming the alluvial region from overflow can judiciously be anticipated from artificial outlets. They are correct in theory, but no advantageous sites for their construction exist."

5°. *Levees.*—"There are certain theoretical views concerning the effects of levee system which are raised again and again in discussing the subject." * * "It is claimed, since the effect of embanking a river is to confine its sedimentary matter to the channel, that the deposit formerly made on the banks must settle on the bottom, and thus ultimately raise the bed and with it, the high water mark." * * "No change of the kind attributable to levees can be shown to have occurred in any river, and the theory is therefore without any foundation in fact. Diametrically opposed to this is another theory, which, for the Mississippi, is equally erroneous, * * that the increased velocity resulting from the confinement of its flood volume between levees will rapidly excavate to a correspondingly greater depth, thus avoiding any permanent increase in the high water mark. This reasoning, if true, would establish conditions singularly fortunate for the levee system; but unluckily the wish has been father to the thought. Uncompromising facts show that the premises and conclusions are both erroneous for the lower Mississippi. Very numerous soundings with leads adapted to bring up samples of the bottom were made by the Mississippi Delta Survey* throughout the whole region between Cairo and the Gulf. They show conclusively that the *real bed* upon which rests the shifting sand bars and mud banks made by the present river is always found in a stratum of hard blue clay, quite unlike the present deposits of the river. It is similar to that forming the bed of the Atchafalaya at its efflux, and is well known to resist the action of the strong current almost like marble. Clearly then the bed of the Mississippi cannot yield, and if the velocity be increased sufficiently to

* Humphreys and Abbot.

compel an enlargement of the channel, it must be made by an increased caving of the banks, an effect which is not quite so agreeable to contemplate."

I will here interrupt the quotation to allude to the reference to this clay bed of the Mississippi, made by Mr. G. W. R. Bayley.* He says,† "It is claimed on high authority that the clay bed of the Mississippi resists the action of the strong current like marble, also that the bed of the Mississippi cannot yield." In a foot note, Mr. Bayley says, the "high authority" is "Humphreys and Abbot." This reference, I think, is a mistake. I have looked the work of Humphreys and Abbot through without finding it. It seems probable that Mr. Bayley's quotation is derived from the report of the Commission, from that part I have just quoted. But attention is asked to the fact, of his having omitted the word "almost," so that he makes the quotation read—the clay bed "resists like marble," whereas it should read—"resists almost like marble." Even in this last form, it is perhaps too strong an expression and might better have been omitted altogether. Nothing in the report of the Commission depends upon it.

To say the clay bed resists the action of strong currents, as clay is known to do, where the current can act upon the bars and banks, which are largely composed of sand, is all that is claimed. I will return to this subject of enlargement of the channel after I have finished quoting from the report of the Commission, which I will now resume. "In truth, no marked effect of the kind is to be anticipated, owing to the comparatively short duration of the increased discharge; for evidently the levees can produce no effect upon the regimen of the river where the water does not stand over the natural banks." I will again interrupt the quotations from the report of the Commission, to state that the data showing the number of days the river water is against the levees, is given for several floods from Carrollton to Columbus, in "Physics and Hydraulics of the Mississippi."‡ At Carrollton, this period is, on an average, 100 days in a year; at Donaldsonville, 50 days. The number of days rapidly decreases as we ascend the river.

I will resume the quotations. "Hence really the practical effect of the levees will be limited to raising the high water mark, and to slightly increase the caving. Since the absolute amount of the increased flood-

* One of the most eminent engineers of the delta region, in his interesting paper "Levees as a System of Reclaiming Low Lands," page 115. † Page 138. ‡ Page 411.

height does not carry the cost beyond the limits of a remunerative investment, it is the part of wisdom to steadily continue work without indulging in groundless fears that the river bed will rise, or in the equally groundless hopes that it will be sensibly depressed. * *

"The prolongation of the delta into the Gulf by the aggregation of sedimentary matter is also assigned as a cause for the ultimate rise of the bed, and hence a future necessary increase to the height of the levees. A possible secular change of this nature is quite too remote in its effects to merit attention from practical men of the present day, Simple calculation will show that hundreds of years will be required to raise the flood-height at New Orleans, an inch from this cause. In fine, then, we are to conclude that there is no mysterious agency, either favorable or injurious, which may be expected to exert a controlling influence upon the levee system. * * It being certain that the alluvial regions of the Mississippi can only be reclaimed by levees, it remains to consider what experience has taught respecting them. The existing system was begun a century and a half ago near New Orleans, and has gradually extended upward until there are but few points on the river at which it has not been tried. * * The faults" (of the system) "are only too apparent. They are—

"*First.*—Vicious levee organization."

"*Second.*—Insufficient height, in adjusting which, only existing high water marks have been considered, without remembering that there has never yet been a great flood in the river in which the water has not been greatly lowered by immense crevasses which occur with absolute certainty."

"*Third.*—Injudicious cross sections and constructions, which alone would be sufficient to explain many of the frequent breaks, under the combined influences of pressure, seepage, burrowing of crawfish, &c."

"*Fourth.*—Inadequate arrangements for inspecting and guarding."

"*Fifth.*—Faulty location of the embankments, which are often placed so near caving banks as to insure an early destruction. Each of the causes of this failure will be considered in turn."

I will not quote this,* but will invite every one interested in the subject, to consult the report of the Commission and see how these matters are there treated.

* As it occupies 11 octavo pages.

The report of the Commission ends with recommending a plan for protecting the alluvial region of the Mississippi from overflow, premising it with the remark that in their judgment "no practical aid can be derived from any diversion of the tributaries or making artificial reservoirs, that cut-offs are very pernicious, and that artificial outlets, although correct in theory, find no useful application to the Mississippi. The pian consists; *first*, in keeping open the Atchafalaya, and the La Fourche, and if borings shall show it to be safe, in re-opening the Plaquemine; *second*, in a general levee system, extending from the head of the alluvial region to the Gulf, including the valleys of the tributary streams. The requisite laws to be enacted by the several riparian States to give the right of way, to confer the authority to make borrow pits and bench marks, to secure the levee from injury from cattle and hogs running at large, and to order out in times of danger, under suitable penalties for non-compliance, the population residing within a reasonable distance from the levees. The main line of levee to be of sufficient height (as already computed) to restrain the floods, and of the requisite cross section to resist the action of the water. Where reasonable security against caving requires large areas of front lands to be thrown out, protection against ordinary high waters is to be given by low front levees closely following the bends, suitable sluices and gates in such cases to be provided in the front and main levees for the rain-water drainage."

This quotation comprises the engineering features of the plan. The report also gives an outline of the administration of the work, its division into districts, and the manner of regulating each by itself and also as a part of the whole. The report expresses no opinion as to whether corporations, States or general government could best carry on the work, leaving that matter to the legislative body that instituted the Commission.

It should be observed that the report of the Commission presents a practical plan for the whole alluvial region, which is based on plain facts, freed from hypothesis. It is applicable to the whole region at once. Were there money enough, it might all be built in a very short space of time. It accepts an increase of the flood heights under what seems to be the most unfavorable conditions, the ascertainment of which was one of the important contributions of Humphreys and Abbot.

The people in Missouri, Arkansas and Mississippi can go to work under the proposed plan without waiting for a proper beginning in

Louisiana. No plan of protection from overflow, however theoretically perfect, would be acceptable to the people of the Mississippi valley, which had to begin at the mouth and be carried upward.

I will return to the question of the enlargement of the natural waterway in consequence of confining to it all the water which now escapes over the banks. I will waive the question whether the enlargement is to take place by scouring out the clay bed or by the increased width due to caving banks, for the practical end would be the same, the enlarged bed would carry off the increased volume of the water without raising the flood height as well in one case as the other. But obviously, we cannot get the increased scour until we build the levees and close the outlets, so as to confine the escaping flood water. I will take as an example of what must be done, the case of a levee at Natchez, a midway place of the alluvial region. There the river on March 6th, 1851, was level with the natural banks, and on March 31st, was 4 feet above them. This gives a fair idea of the sudden nature of the rises even at high stages, with the river imperfectly leveed. Had the river been perfectly leveed, so that the floods which inundate the whole region 25 to 60 miles wide, were confined to the main channel, the rise would have been more rapid and higher. This is the view held in the report of the Commission. But the theory of an enlarging channel says *no*, the channel would have increased so as to prevent this increase of flood height. Granting everything to this hypothesis of an enlarging channel that can be claimed for it, does any one believe that this enlargement could take place throughout the length of the river between Natchez and the mouth, in the short space of 25 days? Think of, or compute the amount of material that would thus have to be carried away by the river in a few days and thrust out into the Gulf, to keep down a rise of a few inches, if adequate levees were built for the whole region. It must not be forgotten nor kept out of sight, that it is the *whole* alluvial region we are considering. It does not seem reasonable that the enlargement could, even under the most favorable conditions, keep pace with the increasing volume. Does it not seem more probable that the levees *at first* would have to be as high as if the bed was unchangeable, even though, when once the water was actually confined to the channel, the flood heights should afterwards, through many years of erosion, gradually diminish?

Common experience, and all accurate measurements, show that in a rising stage of a stream, as the volume is increased the surface of the

water in the stream rises, and it cannot make any difference where the increase of volume comes from. The increase of volume in the Mississippi below, by closing the Atchafalaya, is just the same as if the volume of water before carried off by it came from a new affluent of the same capacity; or more exactly to indicate what I mean, suppose at a flood stage in the Mississippi, the Atchafalaya just carried off the water brought in by Red River, then the closing of the Atchafalaya would practically add the volume of the Red River to the Mississippi, and cause it to rise. Increased levee heights would then necessarily be needed on the Mississippi below it, unless we can suppose the bed of the Mississippi to accommodate this increased volume.

A belief in such sudden enlargement is not generally held by engineers. Very valuable experience on this point has been gained in the attempts to confine the Red river to one channel, a smaller stream, and therefore more in our control. Some of the results of this experience on this river are given in the report of Mr. C. M. Fauntelroy,* where he states the views he obtained from Mr. W. C. Melvin, C. E., Asst. State Engineer. Mr. Melvin says: "As it would hardly be possible to carry on the work of leveeing both sides (of the Red River) at the same time, it is obvious where the work should be first entered upon. Time and assistance must be given to the river to scour its bed to a greater depth, and for its banks to cave. For this purpose, all the growing timber of every description that is standing within 60 feet of the crest of the banks should be cut away; roots and stumps loosened; in short, everything done to facilitate the caving and scouring process."

It is held by some, this view that more water makes the stream rise, is an hypothesis; disregarding all the experience of mankind, they point to the one fact that the closing of Bayou Plaquemine has not increased the flood height at New Orleans, entirely ignoring the well known fact that there has never been a flood since it was closed that has not caused large crevasses in the Mississippi between the Plaquemine and New Orleans, which crevasses have prevented the effect of the closure ever being felt at New Orleans as an increased flood-height, or as an increase of scouring power.

There is no doubt that closing the Plaquemine was a benefit to planters along that bayou, and it was probably at the expense of the planters on the Mississippi. A similar result will probably attend the closing of the La Fourche, if it is ever permitted.

* Secretary to the Commission of Engineers, &c.

There is a moral point of view to this subject as well as an engineering one, and that engineering is always to be received with caution which benefits its advocates at the expense of, or injury to others or the public. Of such is the engineering of the bridges needlessly obstructing navigation, and the flimsy structures of many kinds by which human life is sacrificed to avarice.

In closing, I would earnestly urge those who would wish to be well informed on the subject of levees on the Mississippi to *study* thoroughly the "Physics and Hydraulics" of that river by Humphreys and Abbot. The question is one too vast to be developed in the limits of an ordinary discussion.

The report of the Commission treats of several points briefly, because they are so fully elaborated and disposed of by Humphreys and Abbot, whose conclusions have been generally accepted.*

Mr. George W. R. Bayley.†—I fully recognize and freely admit the fact that "cut-offs" lower the flood-line of a sedimentary river, although the temporary effect is to elevate it below. The first effect of the recent Vicksburg cut-off was an increased rise below, but it soon ceased, and a reduction will follow. The flood-line at Vicksburg and above and below that point, has been reduced several feet by the Terrapin Neck and Palmyra Bend cut-offs of 1866 and 1867, and this reduction extends as far down as Natchez. The lengthening of the river channel by the caving in of its banks in the bends tends to lower the high water slope, and because the velocity required for the discharge of the flood waters or maximum quantity requires a greater slope, the surface rises until the increased slope is obtained. As the length of the river increases, therefore, the flood line rises. On the other hand, a cut-off shortens the river's length, and the slope, for the time being, becomes too great. It can only be reduced to what the quantity and velocity require by excavating its bed and depressing its surface. This always happens. As the river lengthens itself, the flood rises; when a cut-off shortens it, the flood-line falls.

The quantity of water flowing, its mean velocity and slopes of bed and surface are reciprocally dependent and proportional. The resulting effect, however, of excavating the river bed after a cut-off is to

* The "Report of the Commission of Engineers appointed to investigate and report on a permanent Plan for the Reclamation of the alluvial Basin of the Mississippi River subject to Inundation," from which, frequent and full quotations have been made, was officially published as part of the Annual Report of the Chief of Engineers for 1875-6, pages 536-678 (Appendix, O). † Presented May 25th, 1876.

increase the rate of caving in of the river banks and of the higher cultivable lands near its margins; the lengthening of the river goes on, then, with renewed energy until another cut-off occurs, and so the flood-line rises and falls. If it is financially practicable to protect the river banks from being undermined and from caving in, great good can be realized by making cut-offs and shortening the river's length. In the case of the Rhine, which is comparatively a shallow river, this has been done. Can it be as successfully applied to the Mississippi river? When the river, by becoming too long, elevates its flood-line too much, a cut-off becomes necessary to reduce it, but none are practicable for 200 miles above its mouth.

In the case of Red river, next above and below the city of Shreveport, and down to Loggy Bayou (the outlet of Lake Bisteneau), I am decidedly of opinion that the plan of cut-offs can be applied to very great advantage, and that it should be utilized there. By means of it, the flood-line can be very much lowered, its bordering lands reclaimed, levees in great part dispensed with and navigation very much improved there. Three cut-offs can be advantageously made above Shreveport, and eleven or more others below, above Loggy Bayou.* I recommend the application of the plan of cut-offs to Red river, above Grand Ecore, most earnestly, and advise our Society to advocate it.

I have said that the lengthening of a sedimentary river, by reducing its slope or prolonging it, causes a rise of its flood-line in the effort to maintain its slope of surface. The advance of the river's mouth into the sea has the same effect, and I wish to add a few remarks on this point.

In Louisiana, there are many old outlets of the Mississippi, and secondary outlets from these, which were filled up and cut-off from the river in times long past. I will name a few of them only, for they are numerous. Below New Orleans, left bank, the Bayou Terre Aux Bœuf had a course 30 or more miles in length. It is now but a stagnant ditch or coulé, bordered by old sugar plantations for many miles below its former head, 12 miles below this city. Thence to the Gulf its alluvial banks become more and more narrow, the swamps and marsh lands back approaching nearer and nearer to its margins, until only sea marsh is found. The Metairie and Gentilly bayous, back of New Orleans, have their delta ridges formed in like manner. The Terre-Bonne and Bayou

* See lithograph map, herewith (on file at Rooms of the Society).

Black, right banks, were old outlets of the Bayou Lafourche, or secondary channels, and the Little and Big Caillon's and Chacahonla mere outlets of the Terre-Bonne and Black. Each and all have their deltas, which are high, dry, cultivated lands now, and all were formed by overflow deposits ; all were old outlets or "passes."

In each, as its slope was prolonged and so reduced by the advance of its debouchure, the flood line was gradually raised and the banks were elevated and extended laterally. When the prolongation of the main channel became too great for the quantity flowing, and for the maintenance of the velocity of current necessary for its discharge, then the mouth widened, shoaled and divided ; a division of channels occurred, and new and steeper slopes more nearly such as the quantity in the secondary channels required. The beds and banks of the new channels were elevated by deposits, and this process backed the water up in the old main channel, and caused further deposits in its channel, and flattened, by elevating, its high water slope. Lateral overflows and outlets then caused a rise of its banks. These still further reduced the quantity flowing in the main channel, added to the shoaling and the rise of the flood line. In this way and by this process, the elevation of the bed and banks went on, further and further up stream, until it reached the parent river or the primary outlet and nothing more than a portion of the high flood waters during inundations escaped into these silted-up outlets. After the settlement of Louisiana, even this supply was cut off by means of levees built across the heads of the old outlets, and they became stagnant pools or rainfall drains.

The Bayou Lafourche is now, and has for many years, been going through this very same process, but it has been modified and prolonged by the maintenance of levees above where the lands are too narrow and too often overflowed to be worth reclamation. Sixty miles below its head, where levees 2 feet high were sufficient fifty years ago, levees of 12 feet high are insufficient now unless crevasses occur through them in some places, annually, to relieve, for that year, the flood in the gorged channel. But each relieving crevasse adds to the difficulty year by year, and the flood line rises in the effort to obtain that slope which the quantity admitted into the bayou at its head requires. The head level cannot be raised, for that is the surface level of the Mississippi river itself. There is no remedy but the closure of the outlet next the river by a dyke, and the substitution of slack water

navigation by means of a lock in it; for this contest against nature's laws cannot be prolonged indefinitely. The length of the outlet is too great, and its slope too little, for the quantity of water passing into it at its head.

I now come to the case of the Mississippi river itself, at its mouth. Before the era of levees a large proportion of the flood waters of the great river were lost through outlets and over its banks below its last tributary, Red river, and, probably, considerably less reached the sea through its mouth than now. Even now less passes New Orleans, at high flood, than Columbus, Kentucky; the Atchafalaya carries off the difference, less the quantity lost by evaporation.

Before the settlement of Louisiana, some centuries at least, the prolongation of the river into the Gulf below the present forts (21 miles above the Head of the Passes), had reduced its slope to less than what the quantity flowing required, and the quantity passing to sea was further checked by tidal action. The river widened its section from less than half a mile (opposite the present Forts) to three times that width, and correspondingly reduced its depth. It then divided, right and left, into two main channels, with one lesser channel between. Here, gradually, the surface level at flood time was elevated so as to give steeper slopes to the divided channels. Now, the flood line of the river at the Head of the Passes is nearly 3 feet above the mean level of the Gulf of Mexico. As the distance to sea, southwest and eastwards, is nearly 18 miles, and by South Pass, in a south-southeast direction, 12 miles, the flood line slopes are about 2 and 3 inches per mile respectively, east, southwest and south-southeast. Between the Head of the Passes and the Forts, 21 miles, the average slope of the main river is but 1½ inches per mile (the lower portion still less, not more than 1 inch), and from the Forts to New Orleans about 1½ inches per mile.

Now, it is certain that before the division of the waters of the Mississippi at the present Head of the Passes, the mouth of the river was at tide level, and the river surface slope below New Orleans was greater than it now is. As it discharged less water then than now (because depleted by outlets above), its slope needed to be greater. The increase of quantity and of current velocity, since the leveeing up of the outlet channels below Red river and down to the Forts has enabled the channel to discharge a greater quantity with a less slope than

before; therefore, the closure of outlets under the levee system has had a very beneficial effect and every outlet should be kept closed.

About one-third of the river's volume is discharged through the Southwest Pass, but its slope was becoming too flat, by prolongation, for this quantity; a tendency to widen and divide at its mouth is manifest. Its discharge needs to be increased to compensate for its reduced slope.

About 55 per cent. of the river's discharge has been passing out to the eastward through Pass a l'Outre, Southeast, Northeast and Balize Bayou Passes and their subdivisions, the discharge through which has been checked and their division hastened by the prevalent easterly winds and storms. The repeated division of the waters flowing to the eastward, and the consequent tendency to increase the slopes of the divided channels by elevations of bed and surface, has retarded the flow into them and favored and increased flow down Southwest Pass, because of its having but a single channel, and down the South Pass because of its steep slope, directness and short length. The Head of Pass dyke or jetty, built out into the edge of Pass a l'Outre, up stream from the east side of the head of South Pass, diminishes the flow down Pass a l'Outre, and adds to it down South and Southwest Passes, probably more down the latter at present than down South Pass, thus increasing their currents and channel-making power and compensating for extension. By encroaching, gradually, still further upon Pass a l'Outre, and diverting still more of its water westward—the navigable channels to the eastward are useless now—both the Southwest and South Passes may be improved. The effect of the east dyke is to prolong, further up stream, the 2-inch per mile slope of Southwest Pass, but the 3-inch per mile slope of South Pass cannot be extended, obviously, so far up. Hence the South Pass slope should and must be extended into or to a junction with the Southwest Pass slope, below its head, for on that side the fall is greatest, because the distance (and from deep water to deep water as well) is least. Flowing water will always seek the shortest route, if it can, where the fall is greatest. I, therefore, conclude that, at the head of South Pass, the best channel into it is and will be found on the west side of the island in the head of the pass, and that the east channel should be entirely closed by a dyke across to the island. Better water can be maintained in one channel than in two, and one of them should be closed for that reason. Because the shortest distance from deep

water to deep water, and therefore the most rapid slope exists on the west side, that side is the best for developing a channel into the South Pass. If we work in accordance with natural laws, we cannot go astray.

Again, South Pass loses 23 per cent. of its water through Grand Bayou, about half way down from its head, hence the surface slope of South Pass must be more rapid below than above Grand Bayou. We have, by perfecting the prolongation of South Pass about 2 miles seaward by jetties extended (it was partially extended by side shoals and reefs previously), and thereby flattened its slope. To compensate for this, we should increase the quantity of water flowing to sea between the jetties by closing Grand Bayou, and so provide for the prolongation. I also think that the width between the outer jetty ends should be less than above, in order to concentrate the discharge there and excavate thereby a deeper channel in the outlet to sea. By reducing the width we will gain in depth, and maintain the current velocity further out into the Gulf.

Because the great outlets, the "Jump," 11 miles above the Head of the Passes, and "Cubitt's Gap," 4 miles above, lessen the quantity of water in the main river, above the passes, and thereby reduce the velocity of current and channel-making power (as is evidenced by the shoaling above the Head of the Passes, from 10 to 12 feet across the whole river since 1838), these should be closed, and for the same reason, the great Morganza and Bonnet Carre outlets, between the mouth of Red river and New Orleans, should also be closed. Every cubic foot of water which passes the mouth of Red river, or enters the Mississippi there, should by all means be confined to the river channel until it reaches the sea through the river mouths proper; none should be diverted.

The greater the quantity flowing, the greater the velocity, the deeper the section, the less the frictional resistance to motion, and the less the surface slope required for the discharge of a given quantity in the same period of time.

CXXVI.

CUT-OFFS ON THE MISSISSIPPI RIVER.

THEIR EFFECT ON THE CHANNEL ABOVE AND BELOW.

By Caleb G. Forshey, C.E., Member of the Society.

Presented June 17th, 1876.

Of the several problems that have presented themselves for the solution of the practical engineer, in the treatment of the physics of the Mississippi river, none have been more obscure than the effect of cut-offs. Their application to the great desideratum—the redemption of the alluvial lands from overflows—has been, and is yet advocated, by those who believe the straightening of the river would prove beneficial. Fortunately they are few and newly acquainted with these grave questions.

The first cut-off was that known as Shreve's, at the mouth of Red river. It seems to have been made in the interest of navigation, and very little importance attached to the work, in respect to its influence upon the regimen of the river. But from that time forth—1832—there has been a discussion upon this method of redeeming the lands from inundation. As one of its advocates, I made a series of measurements and observations upon the results, to influence the Legislature to effect the cut-off at Racourci. The facts are of record and I re-state them here.

Just below Lat. 31° North, the river made a bend, northward and westward, at about 8 miles it received Red river, at the fenders of the bend, and sweeping around southwardly 3½ miles, sent off the Atchafalaya; thence it returned nearly to the place of departure, after a circuit of about 18 miles. The neck was very narrow, not more than 800 feet across. Capt. Shreve, under authority of the United States, by a

little trench guided the water through, and in a short time the cut-off was accomplished. The river established its new channel by the shorter route.

As soon as the river had time to accommodate itself to the new channel and assume a new and permanent regimen, in 1844, I measured the water marks, and ascertained the levels of a flood approaching that of 1828, which, it will be remembered, was higher than any other recorded in river annals. Then the next in height was that of 1844. In the 16 years of interval the river had time to assume the supposed new regimen.

SHREVE'S OR RED RIVER CUT-OFF.—At the mouth of Red river the fall was 3 feet, by the marks, both of which were very plain upon the trees. At Vidalia, 65 miles above, the fall was 8 inches, on a locust tree upon the batture. It was 3 inches at Waterproof, 105 miles above the cut-off. There was no mark above this point, but I assume that the two floods reached the same elevation at St. Joseph, 120 miles above Red river.

Then below the cut-off, at Morgan's Point, 28 miles below, the depression was 18 inches, by the testimony of Col. Morgan himself; and at Bayou Sara, 40 miles, by my own observation, the mark of 1844 was 3 inches below that of 1828. Beyond that point I had not any bench-mark or testimony, but presume that it expired at Profit's Island, 60 miles below the cut-off.

RACOURCI CUT-OFF was made by the State Engineer in 1848-9. It abridged the channel distance by 24 miles. Its location was but 3 miles below Shreve's cut-off. A canal, some 20 feet in width and 1 mile long, was made in 1848; but the drift wood floated in it, and the river failed to go through. In 1849, the woods were cleared away for 100 feet on each side, the canal was dug deeper to the sand, below the blue clay; some gunpowder was used in blasting, and ultimately, when the water ran through the canal freely, a flat-boat was fitted with a wheel that worked rapidly. This was very effective. The blasting had loosened the earth, and it commenced caving very suddenly. The scene is described as one of terrific grandeur as the widening reached the lofty forest. The falling of trees and the whirl and boil of eddies were truly sublime. In two hours' time, it was a river. The Natchez, Capt. Tom Leathers, with some degree of recklessness, put her head into the tide, steamed through the rushing current and the terrible tempest of falling cypresses. After this, the cut-off was established and the boats generally passed through the shorter channel, though it was the end of the season, before the great Mississippi lay snugly in the new bed thus prepared by the hand of man.

The observations of the Delta Survey, made in 1851, were commenced but two years after this event, and before it had effected much in its new state. Its first effect was to transform the region about the mouth of Red river into a habitable country. Its effect in lowering the water was added to that of Shreve's, making the abridgment of distance about 24 miles,* and added to the shortening effect of the previous cut-off, amounted to 42 miles. The joint effect upon depression of water was equal to 6 + feet.

The effects were felt on the force and direction of the currents upon the banks of the river. The ravages commenced immediately above and below the cut-off, and especially in the direction of the points of attack, where caving had never been experienced before.† The whole community, with one voice, cried out against such tampering with the river. The policy opposed to cut-offs was established, and since they have not been seriously advocated, except by some newly arrived engineer or planter, whose experience was not cognizant of these disasters. The policy is likely to remain, unless the councils of men newly introduced to the history and habits of the river, shall prevail to disturb it.

TERRAPIN NECK AND PALMYRA.—In 1866, nearly 17 years later, Terrapin Neck, which had threatened for many years, was, by the aid of artificial means, cut-off, and abridged the river's distance by 14 miles. It occurred at Lat. 32° 30′ North. Palmyra cut-off occurred in the following year, at Lat. 32° 10′ North; only 20′ latitude intervened between them. Palmyra abridged the channel by 20 + miles.

It will be necessary to consider these two cut-offs together and as one, because their effects were merged in each other so that they cannot be separated. They will, moreover, have to be considered with reference to a new water mark. The flood of 1828 was no longer legible upon the trees, in 1851, and no bench marks had been established. The Delta Survey, under Gen. Humphreys, had to establish an arbitrary bench, which was referred to the mark of 1828, where practicable.

The mark of 1851, which was a high water year, was established by this survey, for future reference. It was preserved and revived by the Commissioners of the Levee Company of Louisiana, in 1871, and in 1872 by the U. S. Engineers, for systematic observations.

* I do not lightly differ from Humphreys and Abbott, but this distance was uniformly called 27 miles. They call it 21 miles. I have revised, and by way of compromise, put the abridgment of steamboat distance, descending, at 24 miles.

† No analysis of the limits of influence was ever made. It is to be regretted; for the 17 years gave it ample time to distribute its effects over a long distance. Its relief was felt at Natchez above and at Baton Rouge below, but how much will never be known.

In 1874, a great flood came, and made fresh marks all over the entire Mississippi alluvion, from Memphis down; and as these two cut-offs occurred midway between the two periods, 1851 and 1867, we shall assume them as reliable, and compare the effects by the two registers. The one is carefully recorded in "Physics and Hydraulics of the Mississippi River," and the other, in the report of the U. S. Commission for reporting a plan for the reclamation of the Delta, as also by the State Engineer. We will state the several differences as shown in 1874, from the mark in 1851.

The city of Vicksburg is about midway between the cut-offs. The register at that city will be taken as the standard. The difference made here by the cut-offs of 34 miles is 72 inches; that is, the water was depressed from 51.3 to 45.3 inches. The difference at Lake Providence, 68 miles above Vicksburg, is 63 inches; that is, the water-mark is reduced from 45.6 feet on the gauge, to 40.37 feet. At Ashton, 88 miles above Vicksburg, it is reduced to 2 feet by the marks upon the trees, as compared with a previous mark, whether of 1851 or 1862 is not certain.

This is the limit of observation from below, and probably comes within the influence of the American Bend cut-off. It would have expired by proportion, at 132 miles distance above Vicksburg.

At Natchez, 84 miles below Palmyra, the greater of the two cut-offs, the depression was 1.5 feet. The slope from 6 feet was gradually observed, though not measured above Natchez. The depression was from 51.3 feet at Vicksburg to 49.8 feet at Natchez, or 1.5 feet. At Red river the effect is still greater. It was reduced from 46.4 to 44 feet, or 2.4 feet. At Batón Rouge, again, it is reversed, and instead of being raised, it is depressed nearly 2 feet. We would place the lower end of the effect at Bayou Sara, 191 miles below Palmyra.*

Thus the cut-offs have depressed the level of discharge for more than 130 miles above and 191 miles below the point where their influence commenced. These cut-offs effected, respectively 30 and 42 inches at their source, making a whole depression of 6 feet; that is, the abridgment of the river by 34 miles, disturbs the regimen for a distance of 423 miles.

But this is not all that we observe. We shall see the ravages upon the banks. At the Terrapin neck, the first effect was to cut away the Hawes Harris plantation, and completely to occupy by the channel of the river, this fine estate. Then attacking the point below, it was carried entirely

* The effect at Ashton was only inferred from the overflow being largely reduced, which effect we put at 2 feet. The crevasse here is now 3 miles wide, and was formerly very deep. It is now so much overgrown with cottonwood that it vents comparatively little water, much less than formerly. It has been open since the war, and is filling itself up rapidly.

away on the left bank. Then crossing over to the right bank, in the Milliken's Bend, it caved in the upper portion of that plantation. It did not stop there, but continued to ravage the two next estates of lower Milliken and Mrs. Maher, and thence continuing, it made havoc of Paw-Paw island. In fact, it has disturbed every portion of the river down to the bend above Vicksburg. In a very short period the effect must be to cut off that very narrow peninsula, and leave Vicksburg entirely an inland city.*

The Palmyra cut-off has in like manner made havoc of Point Pleasant plantation, one of the noblest estates in Louisiana. Thence it crossed over to the Conger place, carrying away the best fields, then reacting upon the bend opposite, the caving commenced upon Ships Bayou and the Alligator Levee, that cost the State so largely, and, far as it was placed back from the front for safety, it is now at the very bank of the river and must be renewed. Then its effects upon Hard-Times must be to carry the river through that bend. The crevasse there this season is the legitimate effect of this cut-off, and all the ruinous consequences upon the plantations of Lake Bruin and lower Lake St. Joseph. It indirectly influences the cavings above Rodney, and the disasters of Waterproof are attributable to it ; and the Kempe estate, whose levee cost the State near $400 000, is legitimately attributable to the same cause.

In general, the effect of every cut-off is to change the points of attack of the river, and with the full force of the fall, due to the shortening of the channel, to precipitate it upon the land and levees that have been reclaimed for many years before. The points of highest improvement are doomed to new servitudes because in the normal condition, these reaches of the river have been least exposed.

Cut-offs as a means of reclaiming the lands are, for all these considerations, so injurious, so disastrous, that they should be guarded against and prevented by legislation and unceasing vigilance.

FLOODS FROM CUT-OFFS.—We should not dismiss this subject without considering the effects beyond and below the depressing of the floods in the neighborhood of the cause.

Below and beyond that influence comes the *raising* of the water, and the elevation of the *flood-line*. Though its first effects occur at the cut-off itself, there must be some limit to the depression. At that place, wherever it be, commences the congestion which produces an elevation

* Since this was written, the cut-off at Vicksburg has taken place—about April 20, 1876.

of the flood. This distance cannot be predicted, but every cut-off illustrates it and travels from it, towards the mouth of the river. In this instance it was 191 miles from the cut-off. The fact, that at Baton Rouge the river was higher than before, is not isolated. It continued the congestion so far as observed. At intermediate places down to Carrolton, in building and repairing levees, they had uniformly to be raised.

At Carrolton, where it was carefully measured, the rise was 0.7 feet where the range was 15.4 feet. At New Orleans, it was 0.7 feet, and at English Turn, 15 miles below New Orleans, it measured 6 inches. At Poverty-Point, it was raised about 6 inches. Judging from the necessity of raising the levee, and below that point, though I had much experience in the repair and renewal of levees and increasing their height, I could not state precisely what the elevation was. But down as low as Tagliaferos, 50 miles below New Orleans, I felt it necessary to raise the levees by a small fraction.

Thus, for 215 miles, the volume of water was congested in the river channel, as I verily believe, by the effect of the various cut-offs. This was doubtless the cumulated influence of all the cut-offs, probably from the Arkansas down to the Racourci; for if it had been the effect of the Racourci and the Shreve's cut-offs it would have shown itself earlier.

It may be stated, then, that the abridgment in channel of 106 miles in the past 50 years, by seven cut-offs, has had the effect to congest the waters by a small but measurable fraction in the channel for the last 260 miles of the leveed river. Its maximum effect may be stated at 2 feet, and this effect is at the upper end of the congestion.

It is believed that this will gradually disappear, as the result of accelerated velocity and enlargement of channel capacity from the confinement of levees and the lashing of steamboat waves. In what period this will occur cannot be predicted.*

* I would take this occasion to retract all the arguments used by me in 1847, to induce the Legislature to authorize the Racourci cut-off. This was done in 1850, but it had not the publicity that I desired, being buried in a public document. For the past 27 years, I have on all occasions used my voice and pen against the resort to cut-offs to prevent the overflows of the Mississippi river.

AMERICAN SOCIETY OF CIVIL ENGINEERS.
INCORPORATED 1852.

TRANSACTIONS.

NOTE.—This Society is not responsible, as a body, for the facts and opinions advanced in any of its publications.

CXXVII.

QUALITIES OF IRON AND STEEL.

A Paper by WILLIAM METCALF, C. E., Member of the Society.

PRESENTED JUNE 15TH, 1876.

I presented to the Seventh Annual Convention of the Society, a paper* referring to the parallel characters of cast-steel and cast-iron, and stating in general terms that all, or nearly all, of the differences we observe in different pieces of steel or cast-iron of good quality, are due to the variations in the proportions of carbon and iron.

Owing to the elaborate and perfect series of mechanical and chemical tests entered upon by the United States Board to "Test Iron, Steel and other Metals," we have virtually abandoned that field.

There are questions, however, which can only be answered by the steel maker and chemist together, and which are quite out of the reach of this Board, except by the aid of the steel manufacturer.

The *first* question that interests the engineer is—do the variations of structure in the ingot by which the steel maker professes to keep his tempers regular, depend upon the variations in the proportions of carbon and iron alone?

Second, if such observed differences of structure depend upon variations in the proportions of carbon and iron alone, are the differences observed caused by such minute changes in the quantity of carbon, that the engineer would be justified in depending upon them in order to have just such quanties of carbon as he should specify in each piece, without submitting the pieces to chemical analysis?

* In discussion of Tests and Testing Machines; Vol. IV., page 272.

The *third* question is—and one as yet entirely unsolved—what is the condition of the carbon in the iron, and what changes take place in the operations of the hardening, tempering, annealing, &c.? We propose hereafter to devote ourselves to this question, and if we succeed in solving it, we will report to the Society; all of the work done heretofore has been merely preparatory to the investigation of this third question; in this preliminary work we have answered the first two questions, and it is the object of this paper to report the results obtained.

In April, 1874, eight ingots were selected from the ingot piles as they were marked for commercial use, by the fracture as judged by the eye, solely with reference to differences of carbon.

Drillings were taken from each and analyses made;* the results are given in Table I.

TABLE I.

No.	Iron by Difference.	Difference.	Carbon.	Difference.	Silicon.	Phosphorus.	Sulphur.	Manganese.	Remarks.		
1	99.554	....	.404	....	.02	.022	Trace.	Undetermined.	Maximum errors.		
2	99.325	.129	.599	.195	.039	.035	.00 002		Silicon.....		.00 002
3	99.143	.182	.789	.190	.026	.040	.00 002		Phosphorus		.00 01
4	99.062	.081	.856	.067	.023	.056	.00 003		Carbon		.00 03
5	98.965	.097	.867	.011	.126	.040	.00 000		Differences.		
6	98.801	.164	.939	.072	.205	.037	.00 000	.00 018			
7	98.745	.056	1.033	.097	.181	.029	.00 600	.00 009		Max.	Min.
8	98.673	.072	1.166	.130	.138	.019	.00 004	Unde-termin-ed.	Iron...	.182	.056
Mean differences..		.0976	....	.0952					Carbon	.195	.011

In March, 1875, about one year after the above selections were made, another series, consisting of twelve ingots, was selected in the same way as the above and analyses made.†

In the series, Table II; No. 1 is one temper below No. 1 of Table I, and No. 12 corresponds to No. 8 of Table I. The interpolations are represented as follows; (Nos. 1, 5, 7 and 10 are interpolations);

* By Prof. Jno. W. Langley, now of Michigan University.

† These analyses were also made by Prof. Jno. W. Langley, except the sulphur determinations, which were by Prof. Albert R. Leeds of the Stevens Institute of Technology, and are inserted here to make the table complete. We cannot say too much of the skill and zeal exhibited by Prof. Langley in these researches.

Numbers of Table	I.		1.	2.	3.		4.		5.	6.		7.	8.
" " "	II.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.

TABLE II.

No.	Iron by Difference.	Difference.	Carbon.	Difference.	Silicon.	Phosphorus.	Sulphur.
1	99.614	.....	.302	......	.019	.047	.0182
2	96.154	.160	.490	.188	.034	.005	.0166
3	99.363	.091	.529	.039	.043	.047	.0186
4	99.270	.093	.649	.120	.039	.030	.0118
5	99.119	.151	.801	.152	.029	.035	.0159
6	99.085	.034	.841	.040	.049	.024	.0106
7	99.044	.041	.867	.026	.057	.014	.0181
8	99.040	.004	.871	.004	.053	.024	.0124
9	98.900	.100	.955	.084	.059	.070	.0163
10	98.860	.040	1.005	.050	.088	.034	.0126
11	98.752	.108	1.058	.053	.120	.064	.0064
12	98.834	—.082	1.079	.021	.039	.044	.0041
Mean differences.		.0753		.0647			

Differences.	Max.	Min.
Iron ...	.160	.004
Carbon.	.188	.004

These tables prove:

First.—That the differences of structure in the cast ingot observed by steel makers, are due entirely to minute variations in the quantity of carbon in the ingots, and to nothing else.

Second.—That these differences are so minute, that actual uniformity for all practical purposes, either for the shop or for construction, is easily attainable. Even the minute difference of .004 between Nos. 7 and 8, Table II, is apparent in the ingots, but we are satisfied with the mean of the twenty—.07995 *of one per cent.*—as being quite near enough for all practice.

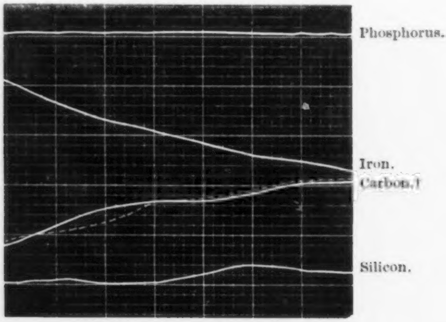
Third.—That when the engineers of the country shall have received from the Government Commission all of the facts in regard to the strength of various grades of steel, the manufacturers will be ready to meet them with all of the exactness in working that is desirable.

Fourth.—That the foreign method of guessing at the carbon in steel by operating upon the bar by bending, nicking, &c., is utterly unreliable as compared to the method here illustrated.

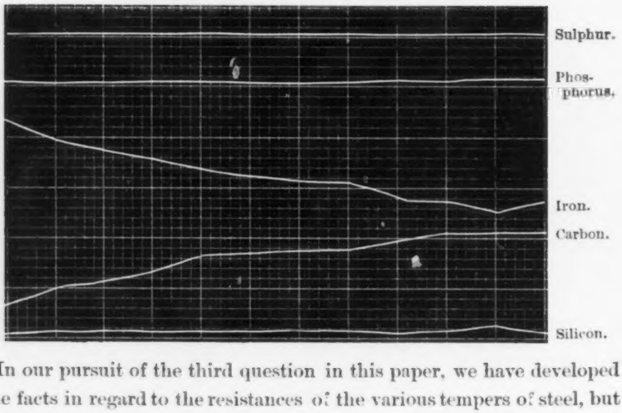
Below are diagrams, showing the relations of the different elements in these two series.* The break in the column of differences of iron in Table II is clearly due to the abnormal amount of silicon in No. 11, from, doubtless, some accident in taking the drillings.

SERIES OF EIGHT, TABLE I.

Sulphur, too minute to plot.



SERIES OF TWELVE, TABLE II.



In our pursuit of the third question in this paper, we have developed some facts in regard to the resistances of the various tempers of steel, but they are too crude to be scientific, and we have no desire to trench upon the work of the Government Commission.

* Each small square represents *vertically* one-tenth of one per cent., and each large square represents *horizontally* one number of the tables.

† The dotted line shows series of *twelve*, reduced to *eight*, by the same commercial numbers as were used in series of *eight*.

CXXVIII.

ON THE FORM, WEIGHT, MANUFACTURE AND LIFE OF RAILS.

Final Report by ASHBEL WELCH, C. E. (Chairman of Committee), Member of the Society.

PRESENTED JUNE 15TH, 1876.

The members of the Committee* on Rails have not much to add to what they have already said. They refer the Society to their first report, and to the Memoir by the Chairman, accompanying it, presented June 10th, 1874; † to "Notes on the Weight of Rails and the Breaking of Iron Rails," by Octave Chanute, one of the committee; ‡ to the second report, presented May 5th, 1875, § and to a valuable contribution by Mr. Frederick J. Slade, published in the *Railroad Gazette*, November 27th, 1875. For a very easy method of comparing the practical values of rails or other things of different durability, they refer to a paper by the Chairman, on the "Comparative Economy of Steel and Iron Rails," published in the Journal of the Franklin Institute, July 1870 ‖ and its formulae made use of in the first report of the committee.

* Appointed January 8th, 1873, to determine the "best form of standard rail sections of this country; the proportion which the weight of rail should bear to the maximum loads carried on a single pair of wheels of locomotives or cars; the best methods of manufacturing and testing rails; the endurance, or as it is called, the 'life' of rails; the causes of the breaking of rails and the most effective way of preventing it, and the experience of railways in this country in the use of steel rails."

The committee consists of Ashbel Welch, of Lambertville, N. J., M. A. Forney and Octave Chanute, of New York, and I. M. St. John, of Richmond, Va.

† Vol. III, page 87. ‡ Vol. III, page 117. § Vol. IV, page 136. ‖ Pages 22-33.

The Committee has been disappointed in obtaining answers to the interrogatories sent to the members of the Society; which answers were expected to furnish much valuable information. It is believed however, that the fullest information would not change, or materially modify any of the conclusions at which the committee has arrived.

It was intended to recapitulate some of the principal points contained in the former reports, but on making the attempt to do so, it is found that no adequate synopsis can be made, without transcribing nearly the whole. We therefore ask those who think it worth while, to read the reports in connection with this.

In these reports, little is said about the manufacture of rails, our mechanical associate not being able to meet with us. That subject really belongs rather to the mechanical than to the civil engineer. Mr. Slade, in the paper referred to, has given some valuable information in the matter.

The 53 pounds steel rails, spoken of in the former reports and in the Memoir referred to, with height and base each 4 inches, head $2\frac{3}{8}$ inches wide and $1\frac{1}{4}$ deep, edges of base $\frac{3}{16}$ inch thick and slopes of the top of the base and under side of the head of 14°, laid down by the Chairman of the Committee in 1867 and 1868, in the worst places for destruction of rails that could be found on the roads between Philadelphia and New York, and where iron rails lasted only four months, have ever since carried a heavy traffic at high speed, and are still apparently as good as ever, except in loss of metal in the head. On straight lines, after carrying a gross weight of perhaps 50 000 000 tons, mostly at high speeds, the heads have lost about 6 pounds per yard, or are worn down a quarter of an inch. On some sharp curves, the sides of the heads were so much worn that the rails have been recently taken up. After the consumable part of these rails is worn off, the residual part, slender as it is, has never failed. When these rails were ordered in 1866, John Brown & Co. refused for months to roll them, partly because they supposed such rails would be worthless, and this was the general opinion, until experience had shown that such was not correct.

These facts show that the rather less slender forms recommended by the Committee are abundantly strong. Though such forms were formerly condemned, they seem now to be generally approved, and, with different but not very material modifications, extensively adopted. The saving by adopting these forms, rather than the thick stems and bases

formerly used, will probably amount, for all the roads laid with heavy rails in this country, to thirty or forty millions of dollars.

Doubtless other engineers have been led by similar reasons to adopt forms similar to those recommended by us. For instance, Mr. Robert Anderson has for some years been using on the Toledo, Wabash & Western Railway, a 52 pounds steel rail, having considerable resemblance to the 53 pounds rail before mentioned, without being aware of the existence of the latter.

Respectfully submitted for himself and his associates.

Ashbel Welch, Chairman.

Addenda to report.—Fish plates, and also the ends of the rails should be carefully gauged and when necessary, filed off, before leaving the mill; so that each plate shall *accurately* fit each rail end. Otherwise a little roughness from the saw bur or otherwise, will often prevent a perfect fit of the slopes of the plate to the slopes of the rail head and base, which the track layer cannot be relied upon to remedy. If it does not fit accurately, the splice will work and wear, and the end of the rail be battered.

It is said that in 1875, 30 per cent. of all the breakages of rails in the United States occurred in January and 50 per cent. in February. Probably many cracked or were strained when the weather was coldest, but were kept in place by the frost, and then came apart when it thawed and the road bed yielded.

The Chairman, differing from some of his colleagues, is of opinion that the fish plate should not be curved, so as to be concave towards the stem of the rail, and that it should not extend outside the base of the rail, so as to have a bearing on the tie.

Probably higher and more durable steel could be used for rails in countries where the frost does not heave the road bed, than would be safe among us. In view of this, the maximum percentage of carbon is put down in the Report of 1875, at 0.55 per cent.

Discussions of Subjects presented at the Eighth Annual Convention.*

ON RAILROAD ACCOUNTS AND RETURNS.†

Mr. Albert D. Briggs.—I cordially approve of the suggestion contained in the able paper just presented. The subject is of great importance to members of this Society and perhaps of still greater, to the general business community.

The railroad companies have received great and valuable franchises from the Legislatures of the several States, and for which they are in duty bound to pay a fair equivalent, by assisting in the transaction of the business of the country, by transporting persons and the various products of the earth at a fair price for the service performed. Now, we shall never know what is a *fair price* for doing work of this character, until we know what it *costs to do it,* and we shall never know what it costs to do it till the accounts of the various companies are kept in a clear, open manner, and, so far as is possible, upon a uniform method or plan. *Uniformity* and *publicity* are the vital factors in the solution of this problem. To accomplish this, it is important that the returns required from the various railroad companies in different States of the Union should also, so far as possible, be uniform in character and detail.

Some roads extend through, or into several States, and should be required to make similar returns of traffic, earnings and expense to the Legislatures of each State through which they run, or into which they extend; but at the present time, the laws of no two States are alike upon this subject, and some of them require no returns whatever to be made.

The Railroad Commissioners of Massachusetts have, for several years—notwithstanding their efforts to make their Annual Report as reliable as possible, by careful tabulation of the returns of the various railroads of that State—felt obliged to caution the public in regard to the unreliability of any general conclusions based upon the reports, in consequence of the lack of uniformity in which the *accounts* of the several companies were kept, and it was only during the last year that the necessity of a radical change in this respect was made apparent to the members of the Legislature. It was exceedingly difficult to eradicate the old and firmly-rooted idea that the *accounts* of railroad companies are matters of no proper concern to the public, and with which the public must not meddle. This idea must be eliminated from the minds of legislators before any radical reform in the laws can be hoped for.

* Continued from page 316. † Referring to—CXXIV; On Railroad Accounts and Returns W. P. Shinn, page 215.

The Railroad Commissioners of Massachusetts prepared a bill, quoted below,* which was enacted into a law by the recent Legislature of that State, and from which good results are confidently expected.

The railroad managers, as well as the general public, are beginning to appreciate the advantages and the necessity of such a law. If one manager operates the road under his charge more economically and successfully than another, it is for his interest that it should be known—be made public—that the public may get the benefit of having the best mode of management generally adopted.

I hope the motion for the appointment of a "Committee on Accounts and Returns," as suggested, will prevail, and that the power of the Committee may be somewhat enlarged, so that it be authorized to correspond officially with the Railway Commissioners of Great Britain upon this subject.

While the returns of the British railway companies are, in many respects, made with great care, especially in relation to passenger traffic, they do not give any report whatever, so far as I know, of the "ton mileage" of freight traffic. This important omission deprives us of the

* [CHAP. 185.] AN ACT to secure greater Publicity and Uniformity in the Accounts of Railroad Corporations. *Be it enacted, &c., as follows:*

SECT. 1. The Board of Railroad Commissioners shall, before the first day of September, eighteen hundred and seventy-six, prescribe a system upon which the books and accounts of corporations operating railroads, or street railways, shall be kept in a uniform manner.

SECT. 2. It shall be the duty of the Board of Railroad Commissioners, from time to time in each year, to examine the books and accounts of all corporations operating railroads, or street railways, to see that they are kept on the plan prescribed under authority of the preceding section, and statements of the doings and financial condition of the several corporations shall be prepared and published at such times as said Board shall deem expedient.

SECT. 3. The Board of Railroad Commissioners is hereby authorized to employ, at a compensation not exceeding twenty-five hundred dollars a year, to be paid as provided in sections seventeen and eighteen of chapter three hundred and seventy-two of the acts of the year eighteen hundred and seventy-four, a person skilled in the methods of railroad accounting, whose duty it shall be, under the direction of said Board, to supervise the method by which the accounts of corporations operating railroads, or street railways, are kept.

SECT. 4. On the application in writing, of a director or of any person or persons owning one-fiftieth part of the entire paid-in capital stock of any corporation operating a railroad, or street railway, or the bonds or other evidences of indebtedness of such corporation equal in amount to one-fiftieth part of its paid-in capital stock, the Board of Railroad Commissioners shall make an examination into the books and financial condition of said corporation, and shall cause the same to be published in one or more daily papers in the city of Boston.

SECT. 5. The Board of Railroad Commissioners shall further have, at all times, access to the list of stockholders of every corporation operating a railroad, or street railway, and may, in their discretion, at any time, cause the same to be copied, in whole or in part, for their own information or for the information of persons owning stock in such corporation.

SECT. 6. A corporation refusing to submit its books to the examination of the Board of Railroad Commissioners, or neglecting to keep its accounts in the method prescribed by said Board under authority of this act, shall be liable to the penalties provided in section one hundred and seventy-four, of said chapter three hundred and seventy-two, of the Acts of the year eighteen hundred and seventy-four, in the case of the neglect or refusal to make a report or return. *Approved April 26, 1876.*

means of making any comparison of the relative cost of transportation of freight in this and that country—a matter of great interest and importance to every student of railway science. If the British railway companies can be induced to record and report the "number of tons of freight carried one mile," as well as the total cost of freight traffic, it will be a *stride* toward perfecting the returns of the railways of that country.

Mr. Albert Fink.—I fully concur in all that has been said by Mr. Shinn on the subject of railroad accounts. $230 000 000 are annually expended in this country in the operation and management of a property that has cost some $4 000 000 000, but there is no one who can tell with what degree of economy this property is being managed. From the want of a proper and uniform system of account keeping, we are without the data upon which the most important questions of railroad economy have to be decided. We cannot tell what is the minimum cost of transportation on any one road under the existing conditions under which it has to be operated. The published reports of railroad companies throw no light upon this subject. A mere statement of the gross earnings, or of the expenses under the usual general accounts, or a statement of the percentage of expenses to earning, is of no value whatsoever to determine the economy with which a road has been operated. That percentage may have been low, yet we have no evidence that it might not have been much lower. On some roads, the operating expenses may exceed the earnings and yet that road may have been operated with the greatest possible economy. The cost per train mile cannot be made a criterion of economy. On some roads, $2 per train mile may be the lowest possible cost, and on others $1 per train mile may be a wasteful expenditure. The cost per ton mile gives no measure of economy. On some roads it may be less than one cent, and on others as much as 10 and 20 cents, yet the roads on which it is highest may have been more economically managed than those on which it is lowest.

To prove that a railroad has been operated with the greatest possible economy—and every railroad manager should be able to make such a proof, not only for his own instruction and satisfaction, but also for the satisfaction of the proprietors of the roads—it is necessary to analyze separately each item of expense, ascertain that it has been reduced to a minimum, and the work performed for the expense incurred made a maximum under all the existing conditions influencing cost and work.

To be able to make such an analysis, it is absolutely necessary that the accounts should show each item of expense separately and also the work performed for it. Comparisons between the operation of different roads

cannot be instituted between the general results, but must be made between the separate items of expense, taking into consideration all the conditions influencing cost and work. It becomes, therefore, necessary if we desire to draw any deductions from the experience of a number of railroads, regarding the conditions under which the minimum cost of transportation can be attained, that there should be uniformity in keeping the accounts.

I understand that it is Mr. Shinn's object in presenting this paper to the Society, to secure a more correct and uniform system of railroad accounts. In furtherance of this object, I herewith submit two statements, marked *A* and *B* (pages 334–336), on which I have shown the headings of accounts which were adopted on the roads formerly under my management, and in which some of the suggestions made by Mr. Shinn are already embodied.

In Statement *A*, 65 separate accounts are mentioned, under which the total operating expenses, as well as the operating expenses per mile of road and per train mile for the separate items are to be shown. In Statement *B*, under 103 accounts, the amount of work performed and the cost of units of work, are to be shown.

The expense accounts, Statement *A*, are arranged in three classes, according to the character of the expenditures.

1. *Maintenance of Road and Buildings and General Expenses* includes all expenditures which do not vary with the amount of transportation work performed, but have to be incurred regardless of it. These expenditures are measured by the unit of "mile of road."

2. *Station Expenses* includes all expenses incurred in handling and warehousing freight and furnishing proper accommodation to passengers. These expenses are independent of the length of haul and must be measured by the number of tons of freight handled at stations or the number of passengers accommodated.

3. *Movement Expenses* include all expenses incurred in moving freight and passengers from one place to another. They are the transportation expenses proper, and must be measured by the unit of weight—or number of passengers—and the length of haul, "the ton mile," or passenger per mile.*

* In the Annual Reports of the Louisville & Nashville Railroad Co. for 1873-4, I have more fully expressed my views on the subject of railroad accounts. I could only repeat here what I have said there, and must refer those who desire to investigate the subject, to that report which fully illustrates my views.

An extract from this report, referring to railroad accounts, was published in pamphlet form by the *Railroad Gazette* (*New York*); for a summary, see Proceedings, Vol. I., page 69.

Statement A.

Operating expenses and Cost per Train-mile and Cost per Mile of Road.

Under these heads the following items are to be arranged :—

Total operating Expenses.			Cost per Unit of Work.			
			Cost per Train-mile.		Cost per Mile	
Passenger.	Freight.	Total.	Passenger.	Freight.	Passenger.	Freight.

No's. Maintenance of Roadway and Buildings.

1 *Road Repairs per Mile of Road and Train-mile*—Adjustment of track.
2 Labor repairing ballast.
3 Material for ballast.
3½ Total adjustment of track.
4 Ditching, labor.
5 Ditching, train expenses.
6 Culverts and cattle-guards.
7 Extraordinary repairs (slides, etc.)
8 Repairs of hand and dump-cars.
19 Repairs of road-tools.
10 Road watchmen.
11 Fencing.
12 General expenses of road department.
12½ Total cost of roadway.
13 Cross-ties replaced, value.
14 Cross-ties, labor replacing.
15 Cross-ties, train expenses hauling.
15½ Total cost of cross-ties.
16 Bridge superstructure, repairs.
17 Bridge watchmen.
18 Shop-building repairs.
19 Water-station repairs.
20 Section-house repairs.
20½ Total cost of bridge and building repairs.
21 General superintendence and general expenses of operating department.
22 Salaries of general officers, main office.
23 Taxes.
24 Insurance.
25 General expenses, main office.
26 Rent account.
27 Advertising and soliciting passengers and freight.
27¼ Total.
27½ *Total cost per mile of road for maintenance of roadway and buildings.*
27¾ Total cost per train-mile for maintenance of roadway and buildings.

Station Expenses per Train mile.

28 Labor loading and unloading freight.
29 Clerks, loading and unloadnig freight, billing, claims, reports, and charges clerks.
30 Agents, cashiers, chief clerks and depot watchmen.
31 General expenses of stations, lights, fuel, &c.
32 Watchmen and switchmen in yards.
33 Expenses of switching—Engine repairs.
Engineers and firemen's wages.
Supervision and general expenses in engine houses.
Oil and waste.
Water supply.
Fuel.
33½ Total.
34 Stationery and printing.
35 Telegraph expenses.
36 Depot repairs.
36¼ Total.
36½ *Total station expenses per train mile.*

Movement Expenses per Train-mile.

37 Adjustment of track.
38 Cutting and replacing rails with old rails.
39 Cost of rails, renewal value.
40 Labor replacing rails.
41 Train expenses, hauling.
42 Joint fastenings.
43 Switches, frogs, &c.
43½ Total replacing rails.
44 Locomotive repairs.
45 Oil and waste used by locomotives.

46 Watching and cleaning locomotives.
47 Supervision and general expenses of engine houses.
48 Engineers and firemen's wages.

48½ Total engine expenses.

49 Conductors and brakemen.
50 Car repairs.
51 Sleeping-car repairs.
52 Sleeping-car expenses.
53 Oil and waste used by cars.
54 Labor, oiling and inspecting cars.
55 Train expenses.

55½ Total car expenses.

56 Fuel used by locomotives.
57 Water-supply.

57½ Total fuel and water-supply.

58 Damage to freight and lost baggage.
59 Damage to stock.
60 Wrecking account.
61 Damage to persons.
62 Gratuity to employees.
63 Fencing burned.
64 Law expenses.
65 Extraordinary damage.

65¼ Total.

65½ *Total movement expenses.*

65¾ GRAND TOTAL FOR MAINTENANCE AND MOVEMENT PER TRAIN-MILE.

NOTE.—In addition to the above, there should be reported :—number of tons of iron or steel used and cost per ton ; of tons of iron worn out per mile of work to gross tons of trains, including locomotive passing over it ; of cross-ties used, and cost per tie.

STATEMENT *B*.

Work Performed and Cost per Unit.

1 Length of road in operation.

Number of daily Trains:

2 Average number of daily trains over road, passenger.
3 Average number of daily trains over road, freight.
4 Total number of daily trains over road, passenger and freight.

Train Mileage:

5 Number of train-miles, passenger.
6 Number of train-miles, freight.
7 Total number of train-miles.

Mileage of Cars in Passenger-trains:

8 Miles run by passenger-cars.
9 Miles run by sleeping-cars.
10 Miles run by baggage-cars.
11 Miles run by express cars.
12 Miles run by postal-cars.
13 Total mileage of passenger-trains.

Number of Cars in each Passenger-train:

14 Number of passenger-cars in each train.
15 Number of sleeping-cars in each train.
16 Number of baggage-cars in each train.
17 Number of express-cars in each train.
18 Number of postal-cars in each train.
19 Total number of passenger, sleeping, baggage, express, and postal-cars in each train.

Dead-weight in each Passenger-train:

20 Weight of passenger-cars in each train.
21 Weight of sleeping-cars in each train.
22 Weight of baggage-cars in each train.
23 Weight of express-cars in each train.
24 Weight of postal-cars in each train.
25 Total weight of cars in each passenger-train.
26 Weight of engine and tender, passenger-train.
27 Total dead-weight in one passenger-train.

Paying Weight in each Passenger-train:

28 Average weight of passengers in each train at 150 lbs. per passenger.
29 Average weight of baggage in each train at 50 lbs. per passenger.
30 Average weight of express in each train.
31 Average weight of mail in each train.
32 Total net weight in each passenger-train.
33 Gross weight of passenger-trains, exclusive of engine and tender.
34 Total gross weight of passenger-trains, inclusive of engine and tender.
35 Percentage of paying to dead-weight (passenger), exclusive of engine and tender.

Passengers Carried :

36 Number of passengers carried one mile, north.
37 Number of passengers carried one mile, south.
38 Total number of passengers carried one mile, north and south.
39 Number of passengers carried in one passenger-car.
40 Number of passengers carried in one passenger-train.
41 Number tons of passengers carried one mile.
42 Number tons of baggage carried one mile.
43 Number tons of express carried one mile.
44 Number tons of mail carried one mile.

Mileage of Freight-cars, and Number of Cars in Train :

45 Miles run by freight-cars loaded, north.
46 Miles run by freight-cars empty, north.
47 Miles run by freight-cars loaded, south.
48 Miles run by freight-cars emp'y, south.
49 Total number of miles run by freight-cars, loaded and empty.
50 Average number of freight-cars in each train, loaded.
51 Average number of freight-cars in each train, empty.
52 Total number of freight-cars in each train, loaded and empty.

Freight Tonnage :

53 Number of tons of freight carried one mile, north.
54 Number of tons of freight carried one mile, south.
55 Total number of tons of freight carried one mile.
56 Percentage of tonnage in direction of smallest to largest traffic.
57 Percentage of paying to dead-weight freight.
58 Average number of tons of freight carried in one loaded car, north.
59 Average number of tons of freight carried in one loaded and empty car, north.
60 Average number of tons of freight carried in one loaded car, south.
61 Average number of tons of freight carried in one loaded and empty car, south.
62 Total average number of tons of freight carried in each car.
63 Net weight carried in one freight-train, north.
64 Net weight carried in one freight-train, south.
65 Average net weight carried in one freight-train, north and south.
66 Dead-weight carried in one freight-train, exclusive of engine and tender.
67 Gross weight carried in one freight-train, exclusive of engine and tender.
68 Weight of freight-engine and tender.
69 Gross weight carried in one freight-train, inclusive of engine and tender.

Earnings :

70 Earnings of passenger-trains.
71 Earnings of freight-trains.
72 Total earnings of trains.
73 Earnings from miscellaneous sources, passenger and freight.
74 Gross earnings.
75 Gross earnings per mile of road, including miscellaneous.
76 Earnings per train-mile, passenger, exclusive of miscellaneous earnings.
77 Earnings per train-mile, freight, exclusive of miscellaneous earnings.
78 Earnings in excess of operating expenses, passenger.
79 Earnings in excess of operating expenses, freight.
80 Total earnings in excess of operating expenses, inclusive of miscellaneous.
81 Net earnings per mile of road.
82 Net earnings per train-mile, passenger.
83 Net earnings per train-mile, freight.
84 Earnings per passenger per mile.
85 Earnings per ton of freight per mile.

Operating Expenses :

86 Operating expenses, passenger.
87 Operating expenses, freight.
88 Total operating expenses.
89 Percentage of passenger expenses to passenger earnings.
90 Percentage of freight expenses to freight earnings.
91 Percentage of expenses to gross earnings.
92 Operating expenses per mile of road.
93 Operating expenses per train-mile, passenger.
94 Cost per car-mile in passenger-train.
95 Cost per gross ton per mile in passenger-train, exclusive of locomotive and tender.
96 Cost per passenger-mile without baggage.
97 Cost per passenger-mile, with baggage.
98 Cost per net ton per mile of baggage, express and mail.
99 Operating expenses per train-mile, freight.
100 Cost per car-mile in freight-trains, loaded.
101 Cost per car-mile in freight-trains, loaded and empty.
102 Cost per gross ton per mile in freight-trains, exclusive of locomotive and tender.
103 Cost per net ton per mile in freight-trains.

Mr. Charles H. Fisher.—The suggestions made by Mr. Shinn in this paper are well worthy of the attention of the Society, for the subject is one of great importance, not only to the managers and officers of railroad companies, but also to the real owners of such properties, the holders of railroad stocks and bonds. To them especially, it is particularly desirable that railway accounts should be kept in such a manner as to afford definite data for determining the real value of the property.

I do not concur in Mr. Shinn's recommendation for the appointment of a committee to correspond with State Boards of Railroad Commissioners, until the subject has first been laid before the railway managers of the country and their assent has been secured.

These State Boards are generally regarded by railroad managers as hostile to the interests of railroad companies, and that this opinion is not entirely unfounded is shown by the discussions in State Legislatures upon the bills authorizing the organization of State Railway Commissions, in reference to the purpose of such commissions, and also by the frequently arbitrary and unwise action of the commissioners.

I believe that if the Society should decide to consider this subject and appoint a committee for that purpose, the committee should first consult with railway officials and learn their views of the nature and scope of the proposed reform. If we act against the wishes of railroad managers our efforts will be in vain. The desired end can be attained only with their assent and co-operation.

Mr. Charles Latimer.—I think Mr. Shinn's paper is most admirable and that it recommends itself to every one interested in railroad matters. It may be, that he goes a step farther than many practical engineers, but as a Society for the advancement of science, our word is "forward."

Mr. Fisher's proposal to carry out Mr. Shinn's idea by obtaining the desired information directly from the roads, is an excellent one. Some roads might object, but there are many that are able and would be willing to furnish all that may be required. It is very desirable that the information be obtained, by a uniform system, from all roads.

For the past 2½ years I have been collecting figures in all branches of the engineering departments on my road,* and as I have had no obstacle in gathering material (all accounts were kept in my office, under my own supervision), I have a mature plan for the data Mr. Shinn desires.

I take up the expenses at the end of October, for the first month of the fiscal year. I have the locomotives and cars weighed, I ascertain the number of tons passing over each division, preserving the total carefully,

* Atlantic & Great Western Railroad; offices, Cleveland, Ohio.

in order to arrive at an estimate of the wear on the rails, and I keep an account of purchases in connection with expenses. There is no cooking of expenses. At the end of each succeeding month, I make up a similar table, so that at the end of the year I have a comparative monthly statement and a complete yearly statement of tonnage, moneys disbursed, etc., etc.* I consider these very important data, and I do not think they are so accurately ascertained any where else in the country.

I have made an effort to obtain similar data from engineers on other roads, and have succeeded in some cases, but I have not undertaken the matter extensively, as it would have demanded too much time. Nevertheless, the information I have obtained has proved valuable in certain calculations as to the wear of rails, to the keeping accurate accounts and has resulted in more economical working of departments.

The proposition of Mr. Shinn, taking in the whole subject, would give us great advantages in establishing points of great value to railway corporations and to the public.†

MR. MENDEZ COHEN.—We have discussed no more important subject to-day than the one now presented.

The whole question of the accounts of railroad companies is one that has been sadly neglected. There is frequently a disposition on the part of officers to cover up expenses in construction or other capital accounts, or at least to furnish no details that are of any practical service. Everything is made to appear to be economical, until some day the stockholders, if not the managers, wake up to realize that there are mistakes, if nothing worse, somewhere.

* The statements are printed each month at a cost of $8.

† Mr. Latimer wrote, June 20th.—I send some of my comparative statements. As far as the engineering department of a constructed and operated road is concerned, the plan I have, which is of my own arrangement, will, if adopted by others, give a very good system of comparison, but there are some faults in it attendant upon the condensation of the subject. I am, however, not wedded to any particular system to such a degree as not to be able to agree upon uniformity.

This statement I have printed every month, beginning at the fiscal year, which is with us October 1st, and I add in the expenses of each succeeding month, so that I have at a glance, at the end of any month, the total expenditure from commencement of the fiscal year, and at the end of the year I have the total expenditure for the fiscal year; the repairs separated from the additions, with credits for all work done, scraps sold and rails collected during that time.

I will add that the charges are honest; nothing is charged to construction which is not absolutely a new construction or addition.

Uniformity in these accounts is a great desideratum, and we will always be lame until a system is adopted by which we may obtain a comparison throughout the land.

I have aimed at this, and have sought to interest other engineers, but the difficulty is principally that the road department is in the hands of men who are not able of themselves to carry out a system, who know nothing of accounts, and rely entirely upon the auditor for information. On the road, it is the engineer who furnishes the auditor his information upon the matter of expenditures in the engineering department.

It will be observed that I get the tonnage passing over the road carefully every month, so that I know to a certainty in regard to the amount of tonnage necessary to destroy the rails. This information is not accurately reached generally.

COMPARATIVE STATEMENT OF EXPENSES IN ENGINEER

Sept., 1874, Thermometer, 87°—46°. Average 67°.

REPAIRS.

Sub-Division.		Length of Road. Main Line.	Sidings.	Total.	Basis of Tonnage. Locomotives, 60 tons ; coaches, 20 tons ; baggage cars, 18 tons ; loaded freight cars, 18½ tons ; empty freight cars, 8½ tons ; cabooses, 8 tons.
1st Div.	1	51.00	10.56	61.56	274 198 ;
	2	50.86	8.87	59.73	274 198 ;
2d Div.	3	42.68	19.55	62.23	Hf. to Xn. 167 325; Xn. to Bd. 292 257;
	4	48.26	13.47	61.73	Bd. to Sb. 292 257; Sb. to K. 171 335;
3d Div.	5	45.80	9.08	54.88	143 318 ;
	6	45.66	9.52	55.18	143 318 ;
4th Div.	7	54.61	7.40	62.01	99 162 ;
	8	49.04	8.65	57.69	Qi. to Sy.; 99 162; Sy. to Dn. 103 161 ;
F.B	9	33.20	6.50	39.70	63 016 ;
M D	10	49.60	24.45	74.05	Sb. to Hd. B. G. 127 597 N. G. 291, 422;
V. B	11	8.03 31.64	13.81	53.48	200 000 N. G.;
N. & N. L	12	33.23	2.20	35.43	25 000 N. G.
		....	......	......	
	....	....	......	.....	
	...	....	......	......	
	....	....	......	......	
	....	....	......	......	
	...	....	......	......	
	. ..	....	......	......	
	....	....	......	......	
	....	543.61	134.06	677.67	

EXPLANATION OF LETTERS IN TONNAGE COLUMS.

Nb. Newburgh ; Hd. Cleveland ; Hf. Meadville ; Xn. Shenango ; Bd. Burghill ; Sb. Leavittsburgh ; K. Kent ; Qi. North Lewisburgh ; Sy. Urbana ; Dn. Dayton ; B. G. Broad Gauge ; N. G. Narrow Gauge.

LABOR.

Classification.	Total No. of Men emply'd on Road Sept., 1875.	Pay Rolls. Month of Sept., 1874.	Pay Rolls. Month of Sept., 1875.	Incr'ase over Sept., 1874.	Decrease from Sept., 1874.	Cost ⅌ mile for Mo. of Sept., 1875.	Total Cost of Labor for 12 Mos. ending Sept. 30, 1875.	I Se 18
Track	110	2 719 51	4 709 10	1 989 59	....	76 50	31 643 75	9 3
"	70	3 258 71	2 484 10	....	774 61	41 60	33 797 22	
"	102	4 023 52	3 680 07	....	343 45	59 14	36 785 12	6 5
"	75	2 617 94	2 771 06	153 12	....	44 90	27 376 06	
"	104	2 406 36	3 556 20	1 149 84	....	64 80	26 437 09	4 3
"	67	2 405 72	2 367 30	....	38 42	42 90	22 306 62	
"	68	3 119 45	2 354 11	....	765 34	37 96	24 536 57	3 5
"	77	2 603 32	1 887 98	....	715 34	32 73	20 504 34	
"	51	1 517 67	1 740 41	222 74	....	43 84	21 385 64	1
"	114	4 433 04	4 041 40	.. ..	391 64	54 58	39 999 21	1 9
"	77	3 358 66	2 669 05	....	689 61	49 90	24 473 99	
"	34	1 627 46	1 100 61	....	526 85	31 07	10 467 95	....
Rail Shop..	21	1 109 02	965 39	....	143 63	1 42	12 443 90	....
Fencing....	38	2 702 89	1 726 73	....	976 16	2 55	16 786 01	....
Carpenters.	63	4 521 10	3 411 47	....	1 109 63	5 03	42 642 19	....
Plumbers ..	5	501 94	303 41	....	198 53	44	3 316 34	....
Water Sup..	22	948 81	895 60	....	53 21	1 32	11 005 55	....
Masons....	26	490 04	1 506 26	1 016 22	....	2 20	6 294 38	....
Painters....	4	286 97	199 42	....	87 55	30	1 462 16	....
Office	6	1 508 34	875 00	....	633 34	1 30	12 121 68	....
Totals......	1 134	46 160 47	43,244 67	4 531 51	7 447 31	63 81	425 785 77	26 (
Credits for months of Sept., 1874 and 1875......	....	....	....	....	....	.....	....	7 (
Credits for 12 months ending Sept., 1875........	....	....	....	....	....	.....	....	....
Decrease........	....	....	....	Labor..	2 915 80	.....	....	....

CONSTRUCTION OR ADDI

Sub-Division.		Main Line.	Sidings.	Total.
1st Div.	1	51.00	10.56	61.56
	2	50.86	8.87	59.73
2d Div.	3	42.68	19.55	62.23
	4	48.26	13.47	01.73
3d Div.	5	45.80	9.08	54.88
	6	45.66	0.52	55.18
4th Div.	7	54.61	7.40	62.01
	8	49.04	8.65	57.69
F.B	9	33.20	6.50	39.70
M D	10	49.60	24.45	71.05
V. B	11	8.03 31.64	18.81	53.48
N & N.L	12	33.23	2.20	35.43
	..	....	......	......
	....	....	......	......
	....	....	......	......
	....	....	......	......
	....	....	.....	......
	....	....	......	......
	....	....	......	......
	....	....	......	......
	....	543 61	134.06	677.67

EXPLANATION OF LETTERS IN DIVISON COLUMN.

F. B., Franklin Branch ; V. B., Vienna Branch ; N. & N. L., Niles and New Lisbon Branch ; M. D., Mahoning Division.

To get material on hand for any division, subtract column of *used* for 12 months from column of *cost of supplies and stock for* 12 *months.*

In column of total cost of supplies, (S) is balance of material carried from previous year, and (P) represents purchases for the 12 months.

Classification.	Total No. of Men	Month of Sept., 1874.	Month of Sept., 1875.	Incr'ase over Sept., 1874.	Decrease from Sept., 1874.	Cost ⅌ mile	Total Cost of Labor for 12 Mos.	
Track......	....	....	....	... .	....	......	756 27	
"	....	....	....	....	....	......	28 68	
"	....	139 54	....	....	139 54	......	1 480 31	1
"	....	107 68	....	....	107 68	......	649 74	
"	....	....	....	....	....	......	711 95	
"	....	52 00	....	....	52 00	......	514 64	
"	....	53 00	....	....	53 00	.....	442 56	
"	....	23 42	1 851 60	1 828 08	....	32 09	3 453 97	
"	....	683 76	....	....	683 76	......	191 43	
"	....	16 50	14 00	....	12 50	19	1 950 62	2 (
"	....	....	....	....	....	......	603 01	
"	....	....	....	....	....	......	3 518 99	....
Rail Shop..	....	....	12 75	12 75	....	02	54 02	....
Fencing....	....	....	....	....	....	......	....	....
Carpenters.	....	1 594 64	833 57	....	761 07	1 21	12 847 51	....
Plumbers ..	....	31 00	....	....	31 00	......	1 079 71	....
Water Sup.	....	42 69	....	....	42 69	......	103 00	....
Masons	....	1 785 95	344 85	....	1 441 10	48	12 505 49	....
Painters....	....	138 95	31 65	....	167 30	04	2 031 36	....
Office	....	820 00	50 62	....	769 38	08	3 236 21	....
Totals......	....	5 499 13	3 138 94	1 840 83	4 201 02	4 63	46 159 47	6 (
Credits for months of Sept., 1874 and 1875....	....	....	....	....	....	......	....	
			Decrease .	Labor..	2 360 19	......	....	....

Total Increase..... .. ..$8 416 39...................

T WESTERN RAILROAD.

Engineering Department, September, 1874-5.

REPAIRS. Sept., 1875, Thermometer, 8[illegible]°—38°. Average 61°.

otal st of abor 12 Mos. nding pt. 30, 1875.	SUPPLIES. Purchased. In Sept., 1874.	Purchased. In Sept., 1875.	Rece'd from other Dep'ts. In Sept., 1874.	Rece'd from other Dep'ts. In Sept., 1875.	Total. Increase over Sept., 1874.	Total. Decrease from Sept. 1874.	Cost of Supplies used from Stock & Purchases. In Sept., 1874.	Cost of Supplies used from Stock & Purchases. In Sept., 1875.	Cost ₩ mile for M'th of Sept., 1875.	Total Cost of Supplies *used* for 12 months ending Sep. 30, '75	Stock carried forward from previous year and Total Cost of Supplies for 12 M'ths ending Sept. 30, 1875.	Total cost per mile for m'th of Sept., 1875.	Total cost of Labor & Supplies, for 12 mo's ending Sept. 30, 1875.
643 75 797 22	9 353 21	5 467 10	633 49	2 735 29	....	1 784 31	13 653 90	8 984 56	74 07	113 215 03	S 4 050 35 P 111 894 08	133 38	177 335 05
785 12 376 06	6 577 34	6 849 28	2 267 59	879 51	....	1 116 14	10 890 30	8 923 71	72 00	83 417 66	S 3 024 53 P 87 562 51	124 03	151 723 69
437 09 306 62	4 394 64	7 878 45	236 70	924 37	4 171 39	....	7 439 11	10 777 72	97 92	74 721 85	S 2 738 87 P 61 062 44	151 75	109 806 15
536 57 504 34	3 550 03	8 972 36	721 63	428 73	5 129 43	....	6 360 60	9 178 78	76 68	64 321 53	S 7 697 04 P 66 349 44	112 12	111 390 35
385 64	181 41	546 26	149 48	105 90	321 27	....	5 062 79	1 543 88	38 88	14 026 09	S738 26 P27 163 78	82 73	48 549 42
999 21 473 99	1 961 88	8 171 66	2 324 14	1 157 56	5 043 20	....	12 299 31	10 769 74	84 45	122 027 50	S 1 905 07 P 109 996 86	137 07	174 470 04
467 95	....	....	132 10	....	....	132 10	972 25	....	....	4 044 80	S 20 77 P 8 164 51	....	18 632 46
443 90	....	....	....	....	....	....	....	....	....	....	....	....	12 443 90
786 01	....	....	....	....	....	....	....	....	....	....	....	....	16 786 01
642 19	....	....	....	....	....	....	....	....	....	....	....	....	42 642 19
316 34	....	....	....	....	....	....	....	....	....	....	....	....	3 316 34
005 55	....	....	....	....	....	....	....	....	....	....	....	....	11 005 55
294 38	....	....	....	....	....	....	....	....	....	....	....	....	6 294 38
462 16	....	....	....	....	....	....	....	....	....	....	....	....	1 462 16
121 68	....	....	....	....	....	....	....	....	....	....	....	....	12 121 68
785 77	26 018 51	37 885 11	6 465 22	6 231 36	14 665 29	3 032 55	56 678 26	50 178 39	74 05	475 774 46	492 368 51	137 86	897 979 39
....	7 519 18	4496 64	....	....	....	....	....	....	....	....	....	....	....
....	....	....	....	....	....	....	....	....	....	Scrap sold	13 671 12 Ruts, &c. 17 059 43		30 730 55
....	....	....	Incre'se	Suppl'is	11 632 74	....	....	....	....	....	....	....	....

ON OR ADDITIONS.

756 27 28 68	831 23	340 78	38 52	299 42	....	228 55	868 75	640 20	5 28	3 199 63	3 012 16	5 28	3 797 11
480 31 649 74	1 347 75	546 74	464 42	70 44	....	1 194 99	1 906 13	617 18	5 00	20 354 34	19 667 65	5 00	21 797 70
711 95 514 64	973 62	22 40	28 48	29 93	....	949 77	1 002 10	52 33	47	3 282 65	2 867 54	47	4 094 13
442 56 453 97	804 32	303 02	326 25	9 00	....	818 55	1 130 57	312 02	2 61	4 614 02	3 487 40	18 08	7 383 93
191 43	40 36	....	213 96	....	....	254 32	470 39	....	....	2 571 46	2 257 28	....	2 448 71
950 62 603 01	2 097 37	7 470 75	49 31	181 75	5 505 82	....	2 146 68	7 652 50	60 00	30 079 16	28 946 16	60 09	31 499 79
518 99	....	....	....	....	....	....	....	....	....	3 195 90	3 195 90	....	6 714 89
54 02	....	....	....	....	....	....	....	....	....	....	....	....	54 02
....	....	....	....	....	....	....	....	....	....	....	....	....	....
847 51	....	....	....	....	....	....	....	....	....	....	....	....	12 847 51
079 71	....	....	....	....	....	....	....	....	....	....	....	....	1 079 71
103 00	....	....	....	....	....	....	....	....	....	....	....	....	103 00
505 49	....	....	....	....	....	....	....	....	....	....	....	....	12 505 49
031 36	....	....	....	....	....	....	....	....	....	....	....	....	2 031 36
236 21	....	....	....	....	....	....	....	....	....	....	....	....	3 236 21
159 47	6 094 65	8 683 69	1 120 94	590 54	5 505 82	3 446 18	7 524 62	9 274 23	13 69	67 297 16	63 434 [illegible]	18 [illegible]	109 593 56
....	360 60	3 538 62	....	....	....	....	....	....	....	....	U. S. R. S. Co.		6 220 47
....	....	....	Incr'ese	Suppli's	2 059 64	....	....	....	Credits.	for12 m'th	Miscellaneous.		5 352 36
....	....	....	....	....	....	....	....	....	....	....	Material on hand		$14 941 65

The resolution suggests a committee to report one year hence. I do not think the time is any too long. There is wisdom in not pressing the matter too rapidly.

Mr. W. Milnor Roberts.—It is certainly important that uniformity in the keeping of railroad accounts should, if possible, become the rule and not remain exceptional; and perhaps the suggestions in Mr. Shinn's paper, if adopted by all the companies, may bring it about.

Under the present irregular and sometimes mystified methods of publishing railroad accounts, or at least the annual fiscal results, it is oftentimes impossible for the stockholder (who, it was formerly supposed had an interest in knowing) to ascertain whether his invested means are really yielding a substantial, or merely a fanciful profit. With clear and honest accounts, kept in the main, as Mr. Shinn has suggested, this individual, whose rights, I think, are entitled to some respect, could at all events have the satisfaction of knowing whether his road was actually making or losing money, and whether a dividend, if any, was fairly earned, or only declared for a purpose—the purpose, perhaps, not being declared.

While leaving to others who may be more familiar than I am, with the modern systems of railroad accounts, to refer to these accounts and report items more in detail, I embrace this opportunity, for and in behalf of many who are not here and who desire correct annual railroad statements, to thank Mr. Shinn for introducing this subject as a theme for discussion in our Society. It is not the special province of this Society to set itself up as a public censor; but it should always be a pleasing duty of a body of men who are presumably familiar with all the manipulations, scientific and otherwise, of this, the most important arm of the world's progress, to aid, in all legitimate ways, its economical management.

While self-improvement, by mutual interchange of acquired knowledge and experience, may be regarded as primary in our Society organization, this is by no means incompatible with the general improvement of the world, so far as sound counsel, based upon intelligent thought, may lead in that direction.

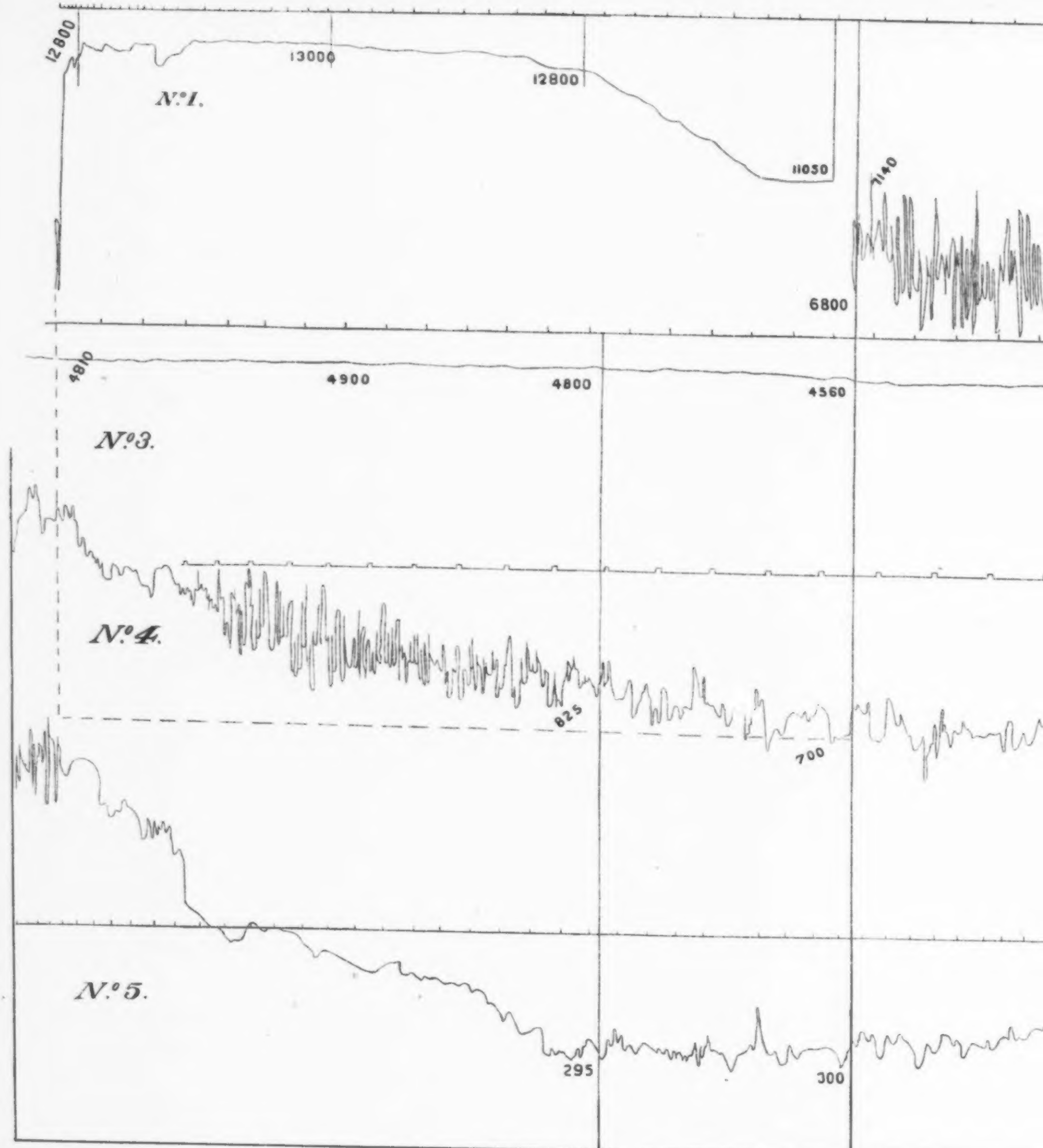
At the first view, I entertained a slight feeling of doubt respecting the advisability, or rather, as to the propriety of this Society's appointing a committee for the purpose indicated by Mr. Shinn; but, on further reflection, it appears to me that the plan proposed takes away the objection that was arising in my mind. The committee suggested by Mr. Shinn is only authorized by the terms stated, "to correspond with the Commissioners of the several States, and to arrange for a meeting with them to

consider the steps necessary to the adoption of this desirable improvement;" namely, securing "Uniform accounts and returns of Railroad Companies to State Commissioners." If this can be arranged without expense to the Society, I would be in favor of its adoption.

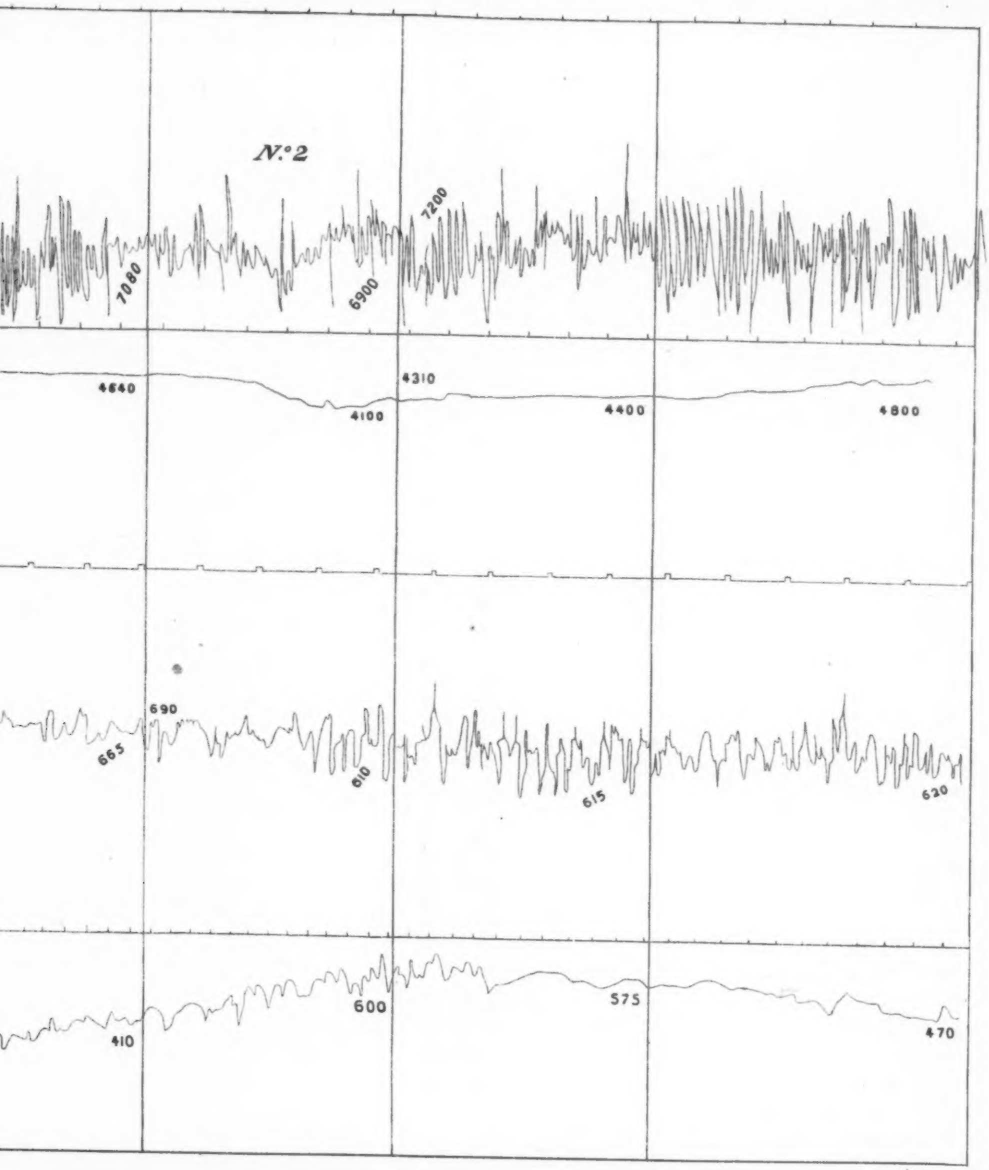
MR. WILLIAM P. SHINN.—I have studied this matter carefully. Mr. Fisher's idea is, that the result ought to be reached by the voluntary action of the railroad companies, on the recommendation of the Society. I think you may as well expect the particles of water at the mouth of the Mississippi, by a little entreaty from the banks, to be persuaded to change their former course, and accomplish the work sought to be effected by the construction of the jetties.

I have had experience with and observations of the accounts of some twenty-five or thirty railway companies in this country, and it is rare to find one whose accounts would give a tithe of the information that is wanted, to determine questions of cost. Now the matter of State reports is not a new one; they are required by some eight or ten States, of which Massachusetts, New York, New Jersey, Pennsylvania, Ohio, Indiana, Illinois, and Iowa occur to me, all of which require these returns to be made to the Auditor General, or to some other appropriate officer. But the usual plan pursued by the Railroad Commissioner (or whatever the officer is called), is to send out blanks containing lists of questions for guidance of railroad companies in preparing a return, showing the business of the various corporations, which, it is generally supposed, comprise all the data necessary to the determination of cost. I am prepared to state that they do nothing of the kind; that is, no intelligible comparison can be made between them. It remains like the old question given to school children, of the comparison of three horses with two cows. There is no mode presented for getting at any particular point, by which all will start from the same premises, or reach the same result.

From my observation and experience, I am firmly persuaded that it will be entirely futile to endeavor to bring about the desired uniformity through any voluntary assistance from the railroad companies. As carried on now in the States named, the result, as I have mentioned, is simply to satisfy the Auditor General's State Commissioners, or other officials, without furnishing data of the least value for comparison, and our efforts would be futile were we to attempt to show them what we want. They can do nothing in the matter but recommend to the Legislature a remodeling of the laws. In that way the matter can be brought about, and I am convinced it can be done in no other.



12800
13000
12800
N.º1.
11030
7140
6800
4810
4900
4800
4560
N.º3.
N.º4.
825
700
N.º5.
295
300


N.º2
7080
6900
7200
4640
4100
4310
4400
4800
665
690
610
615
620
410
600
575
470

RESISTANCES OF RAILWAY TRAINS.[a]

Mr. William P. Shinn.—The Committee[b] on Resistances of Railway Trains has as yet had no meeting, and the facts, which I am able to present in the form of a communication to me, from Mr. P. H. Dudley, inventor of the dynagraph, I will offer more as member, than as Chairman of the Committee.[c] It is as follows :

In compliance with your request, I have arranged some of the data we have obtained in making experiments with the dynagraph[d] upon railway resistances, which is herewith presented upon sheets marked respectively Nos. 1, 2, 3, 4 and 5, and in the tables annexed. (Pages 348-352).[e] The diagrams were taken upon the steel track of the Lake Shore & Michigan Southern R'y, at the Collingwood Yards, some 8 miles east of Cleveland, Ohio. The track is all tangent and ballasted with gravel. It was about $1\frac{3}{4}$ miles long between the switches.

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a Referring to resolution adopted by vote of the Society, canvassed May 3rd, 1876. (Proceedings, Vol. II., page 48, 59)

b Consisting of William P. Shinn, of Pittsburgh, Pa., Alexander L. Holley, of New York, Robert H. Thurston, of Hoboken, N. J., Charles Paine, of Cleveland, Ohio, and Charles H. Fisher, of Albany, N. Y.

c I present them mainly for the same reason and from the same motive that the opening chapters of a serial story are distributed gratis by our leading sensation story papers, to create an appetite for more, with those of our profession interested in the multitude of questions involved in the economy of transportation by railway.

d The dynagraph (as described in another place by Mr. Dudley), is an instrument designed to measure and record upon paper the resistance due to the movement of trains, also to show by the kind of line made, the general condition of the track and motive power. It is fitted into a car which is attached next to the locomotive, and is of the following described general construction. Underneath the car is a steel cylinder filled with oil, having two pistons, one 4 and the other $1\frac{1}{4}$ inches in diameter, so arranged that either one can be used at pleasure. The draw-bar of the car is extended back and draws directly on the piston, which forces the oil in the cylinder through a pipe to a small cylinder, in which is fitted a piston, acting against springs of known tension. The cross-head of the small piston moves the lever, carrying the pencil, which records upon the moving paper the amount of force exerted. The paper used is $10\frac{1}{4}$ inches wide and is in lengths ranging from 150 to 400 feet.

The paper is moved by direct motion from the car axle. It is wound on a drum upon one side of the instrument, and passes through between two steel rollers, over a little table about one foot square, through another set of steel rollers, and thence to another drum, which winds up the paper as it passes through the rollers. Usually one-fourth of an inch of paper is made to represent 100 feet on the track passed over. An electrical chronograph records the time every $7\frac{1}{2}$ seconds, consequently the speed for any given instant can be determined ; it is necessary data in making the calculations.

e Explanations of the Diagrams.—The force line and chronograph line are full size as to length, but in order to get so many diagrams on one sheet, the vertical distances between the zero line, force line and chronograph line are reduced.

Nos. 1, 2 and 3 were taken with the large piston on heavy freight trains.

No. 1 was taken upon the Lake Shore & Michigan Southern R'y, and shows the force required to start a train of 35 loaded cars, one caboose and dynagraph car ; total weight 709 tons ; about 3 000 feet in length are represented. The zero line of force for this diagram is the broken line extended partly through diagram No. 4.

The chronograph line for this diagram is the upper one on the sheet, and the one immediately under it, the record of force required to start the train from the Union Depot at Cleve-

All calculations were made from the diagrams as shown, between the eighth and ninth mile post. The line has two gradients upon it, for which allowance has been made in the calculations. We tried to run all of the cars at speeds of 10, 20 and 30 miles per hour respectively. From the short distance run, it was not possible to acquire the proper speed in each case. After passing the eighth mile post, the engineer did not touch the throttle valve or cut-off, as any changes in these are at once indicated and recorded upon the paper, and we did not wish any change of force due to increase of steam pressure to enter into the calculations of single cars.

The engines used in all these experiments were quite small ones, having single drivers, with the tender and frame rigidly connected; they do not run steadily, but are in constant oscillation.

The difficulties of the case preventing the runs to be made with uniform velocity, required the calculations to be reduced; therefore, we had a column in the sheets of initial velocity, which is that of passing the eighth mile post and one of final velocity, which is that of passing the ninth mile post. The time is indicated every 7½ seconds, so that the space between the time indications shows whether the speed is being

land, Ohio. The engine was a *Mogul*, having 49 600 pounds upon the drivers, and would pull 1 000 to 1 500 pounds more than that shown upon the diagram, before slipping her drivers, upon a good rail. The slightly irregular line at first, is due to a slight movement of the throttle valve by the engineer, while the downward movement of the force line in the last 1 000 feet is due to the speed of the train and setting back the reversing lever. The figures upon the force line show the pounds of force exerted upon the draw-bar to draw the train, per unit of time.

No. 2 represents 4 000 feet in length, run by an ore train upon the iron rail of the Cleveland & Pittsburgh R'y; weight of train, 313 tons. The zero line is the same as that of No. 3, which is also the chronograph line of No. 4. The speed of the train is shown by the upper chronograph line. The vibrations of the force line were caused by a rough iron track, the joints being very much depressed. These vibrations are very much below the average; as many of them were so great, the various ones could not be easily distinguished much less engraved; to check them, we were obliged to use a spring draw-bar.

No. 3 is the same train upon the Lake Shore & Michigan Southern R'y as No. 1, when running along at its usual speed, which is shown by the chronograph line drawn through Nos. 1 and 2. It will be noticed, the force line is quite uniform, which we found to be so when the track (steel rail) was in good condition, well ballasted and the engine in order; but when otherwise, the force line assumes the character of that shown in No. 2.

Diagrams Nos. 4 and 5 are of a series of experiments with two or three cars, and represent starting them and a run of about 1½ miles. The cars were two loaded Empire cars, and with a dynagraph car, weighed 52 055 tons (2 000 pounds). The engine used to draw them is a small one, having a single pair of drivers; the tender was rigidly attached to the engine and when running was constantly oscillating from side to side, which gives the force line a vibratory motion. No. 4 was run at 20.9 miles per hour, and No. 5 (same train) was run at 8.7 miles per hour.

The chronograph line, No. 4 shows as it appears when a train is running fast, though somewhat exaggerated in this.

After the diagrams are taken, they are all calculated and the number of foot-pounds required to move the train for the run or any given distance obtained.

accelerated or retarded and at what points. The cars used were those which were found in the yards and such as were in frequent service.

We cannot explain many anomalies which occur in the results with single cars. In the final resistance given, that due to the air is included. As yet we have not succeeded in making observations upon the wind which give uniform results, though we have tried the most delicate anemometers and vanes. The diagrams in which some of the anomalies occur, do not indicate that anything was wrong with the working of the instrument, yet it is possible that such was the case. The service upon the instrument with single cars was very severe, the strain upon the pipes and joints often exceeding in steady work, 2 000 pounds per square inch, to which is to be added that due to shocks, whence it is very difficult to keep the instrument in proper order without constant care.

We find in all our experiments, when the weight of the cars is partially carried upon the ends of the truck frames, that after passing curves or switches, it oftentimes requires a long distance to be run before the frames will straighten up and not bind upon the flanges of the wheels, causing increased friction which often appears in our experiments. It is seldom that journals upon the same cars are of the same size ; the wear is not uniform, the journals varying from $\frac{1}{16}$ to $\frac{5}{16}$ inches in the amount of wear. Flanges of wheels are often badly worn upon opposite wheels and the axles are not parallel. There does not seem to be any uniformity as to the amount of width given to the bearings—some will have 2 and others, $2\frac{1}{2}$ inches. As a rule, we have found that a bearing of 6 inches in length has less friction than one of only 5 inches in length.

In loaded cars, especially after they have stood some time, the lubricant seems to be forced from between the journal and the brass, so that the cars need to be run a short time or newly oiled, before running at the usual friction. The rate of friction seems to be higher per ton in loaded cars than in empty ones.

We have not as yet tried to formulate any of the information obtained, deeming it of importance to gain more before attempting anything of the kind, and then I think a formula will be necessary for the various kinds of constructed cars intended to do the same work.

One thing was quite noticeable in the construction of some of the brasses of the Lake Shore & Michigan Southern R'y. In order to save a little brass, there is only about 2 inches of bearing upon the top of the box, the ends are made much thinner, which, as they become thin by wear spring away from the journal, increase the pressure per square inch

in the centre of the brass and force out the lubricant, thereby increasing the friction and the liability to heat.

In all of the experiments detailed upon the sheets, petroleum was used as the lubricant, except that of the Baltimore and Ohio car, to which grease was applied. The wheels were the ordinary chilled cast iron, 33 inches in diameter, except those of the Baltimore and Ohio car, which were 30 inches in diameter.

The experiments made with heavy trains are fully as interesting as those with single cars, though the data and calculations are not presented, because so voluminous. We found that with the long and heavy trains of the Lake Shore & Michigan Southern R'y, of 650 to 700 tons, it required less fuel with the same engine (No. 485, *Mogul*) to run trains at 18 to 20 miles per hour than it did at 10 to 12 miles per hour. The engine, at the highest rate of speed, seems to produce its power more economically by using the steam expansively to a greater extent than at the slower speeds. From calculations of a trip from Toledo to Cleveland, of 29 loaded and 2 empty cars, weight 590 tons, the friction at 20 miles per hour as an average the entire distance, was 7.45 pounds per ton, including that due to gravity ; total force expended in the whole distance of 109 miles was equal to 2 528 203 700 feet pounds.

From calculations of a trip from Cleveland to Erie, 95½ miles, of 37 loaded cars, weight 709 tons, the friction at 20 miles per hour as an average the entire distance, was 6.85 pounds per ton, including that due to gravity ; total force expended was equal to 2 498 396 320 feet pounds, exclusive of that required to move the motor itself. From calculations of a trip from Erie to Buffalo, 88 miles, of 25 loaded and 2 empty cars, weight 512.4 tons, the friction at 20 miles per hour as an average the entire distance, was 7.94 pounds per ton ; total force expended was equal to 1 843 736 850 feet pounds.

Diagram No. 1, taken upon the Lake Shore & Michigan Southern R'y, shows the force required to start a train of 35 loaded cars, one caboose and a dynagraph car ; total weight 709 tons ; about 3 000 feet in length are represented. The zero line of force for this diagram is some 4 inches below the one shown on the plate. After the train was in motion, it required a force of only 4 000 to 4 800 pounds to move the train. The line of force on the steel track was very smooth and uniform. The chronograph line for this diagram is the upper one on the sheet, and the line immediately under it, the record of force required to start the train from the Union depot at Cleveland, Ohio. The engine was a *Mogul*, having 49 600

pounds upon the drivers, and would pull from 1 000 to 1 500 pounds more than that shown upon the diagram, before slipping her drivers upon a good rail. The slightly irregular line at first, is due to a slight movement of the throttle valve by the engineer, while the downward movement of the force line in the last 1 000 feet is due to the speed of the train and cutting back the reversing lever. The figures upon the force line show the pounds of force exerted upon the draw-bar to draw the train per unit.

To furnish the power for the trip from Cleveland to Erie, engine No. 485, *Mogul*, was used, consuming 8 425 pounds of coal, each pound yielding 296 545 feet pounds of power to move the train—less than 3 per cent. of the theoretical value of the coal. In some previous experiments, this engine only evaporated 4.66 pounds of water, per pound of coal. The grades upon this portion of the track do not exceed 17 feet per mile, and the curvature is very moderate. To compare the amount of power developed by engines upon other railways, we took a train from Cleveland to Wellsvile, 99 miles, on the Cleveland & Pittsburgh R'y, which has 40 feet grades and very sharp curves. The weight of train was 313 tons (ore train); total force exerted was 1 754 556 400 feet pounds, using 4 400 pounds of coal, each pound of coal yielding 398 763 feet pounds, utilizing in moving the train 4.5 per cent. of the theoretical power of the coal. The average resistance per ton for the entire distance was 10.72 pounds, which was owing to its heavy grades, sharp curves, and an iron track which is far from being smooth. Compare this amount of work done, with that upon the Lake Shore & Michigan Southern R'y, from Cleveland to Erie.

Diagram No. 2 represents 4 000 feet in length, run by the ore train mentioned, upon the iron rail of the Cleveland & Pittsburgh R'y; weight of train, 313 tons; amount of force required to run the train, 6 900 to 7 140 pounds. The zero line is 2⅜ inches below that here represented. The vibrations of the force line were caused by a rough iron track, the joints being very much depressed. These vibrations are very much below the average; as many of them were so great, all could not be easily distinguished, much less engraved; to check them, it was necessary to use a spring draw-bar.

On the Cleveland & Pittsburgh R'y, the resistance per ton is 57 per cent. greater than upon the Lake Shore & Michigan Southern R'y, while the effect or work done per pound of coal is 50 per cent. greater on the former than on the latter. The comparison shows that the effect of the

fine steel track of the Lake Shore & Michigan Southern R'y is in a great measure offset by the better adaptation of motive power upon the Cleveland & Pittsburgh R'y ; therefore, so far as the cost of transportation is concerned, the cost upon each road could be lessened by adopting the better principles of the other.

We calculated the amount of power lost in stopping heavy trains at water stations and grade crossings. For the train of 709 tons, it ranged from 20 000 000 to 40 000 000 feet pounds, depending upon the difficulties of the place ; in one instance is was 35 696 950 feet pounds. Now, dividing this by 296 545, the amount of power developed by one pound of coal, it gives the amount of coal consumed in starting the train, which in this case was 120.4 pounds ; the same amount of power would run the train 2 miles on a level. We made a calculation of the amount of tonnage passed over the Lake Shore & Michigan Southern R'y in 1873, and had they been able to reduce the friction of all trains 25 per cent. it would have effected a saving of over $750 000. Of course, a large portion of this would be absorbed in providing the means for the first year, but still a large saving can be made by a better adaptation of the best details of car construction. A saving of one pound per ton of friction on our vast railway business would save many millions of dollars to all concerned. To do this, does not require an entire revolution in our cars, but slight changes in the brasses and avoidance of many of the now objectionable details of cars.

Signed, P. H. Dudley.

Cleveland, O., June 12th, 1876.

It is my conclusion, at first hastily reached, but confirmed and strengthened by more mature reflection, that the dynagraph is destined to correct many errors, to destroy many cherished and long-established ideas of railway economy, and to establish upon a firmer basis other theories which engineers felt confidence in, but which have been rejected by the practical railroad man as only theoretical, and therefore, unworthy of his attention.

To one or two only, of the points developed by Mr. Dudley will I refer. One, that it takes actually less power to keep a freight train moving at 18 to 20 miles, than at 10 to 15 miles per hour. The dream of many of our most prominent railway managers has been to have separate tracks for freight, so that freight trains could be kept at their most economical speed of 8 miles per hour. This dream is thus shown to be—in one

of its features at least—one in which the promise, if "kept to the ear," would be "broken to the hope," and its realization not an advance in economy of fuel or power.

Again, we see the necessity of greater economy in consumption and utilization of fuel, when, by better use of fuel, a road of 40 feet grades is equalized with one of 17 feet grades. An economy amounting to millions of dollars per annum is not only possible but necessary in conducting the railway transportation of this country, and I hope that the members of this Society having relations with railways, will not fail to bring the matter to the attention of their respective companies, with the view of securing their financial co-operation, without which the work of experimenting with the dynagraph cannot be successfully prosecuted.*

Mr. Joseph B. Davis.—I desire that Mr. Dudley should have all possible encouragement. He has been conducting these investigations by himself, disinterestedly, and solely from a native interest in results to be obtained. So far as I can tell (and I have had opportunity to observe, unknown to him), his has been a work of remarkable value; though undertaken single handed, and with only such aid as he could command by his personal influence, already the experiments are of intrinsic worth. In my judgment, he should henceforth be effectually supported by all who are interested in the economy of railway transportation.

Mr. William P. Shinn.—It is the intention of the Committee to bring this matter before the leading railroad companies, and get them, if possible, to agree to contribute a certain amount per month, to be disposed of, under such regulations as they may see fit. So long as that is done, Mr. Dudley is willing to go on, work out these results, and apply his investigations to such points as the railroad companies wish. There is reason to hope that the necessary funds will be forthcoming, and we may be able to present, from time to time, the results of these investigations.

As many present do not know Mr. Dudley, I wish to say that he is a civil engineer and member of the American Institute of Mining Engineers, though not a member of this Society, and in his profession has been prominent and successful.

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* A report of Committee of the American Institute of Mining Engineers on Railway Resistances, presented June 20th, 1876, with other matter on this topic will be found in "*Railroad Gazette,*" Vol. VII. pages 317, 320.

RECORD OF EXPERIMENTS MADE WITH THE DYNAGRAPH ON LAKE SHORE & MICHIGAN SOUTHERN R'Y, AT COLLINGWOOD, O.

WITH SINGLE EMPTY CARS.

Car.	Number	Railway.	Date.	Journals. Inches. Diameter.	Journals. Inches. Length.	Load including dynagraph car. Tons.*	Velocity—feet per second. Initial.	Velocity—feet per second. Final.	Velocity—feet per second. Difference.	Average speed, miles per hour.	Resistance in pounds per ton. Observation corrected for— Level track.	Resistance in pounds per ton. Observation corrected for— Uniform velocity	Weather.	Temperature.	Barometer.	Wind. Direction.	Wind. Velocity, miles per hour.
Box D.† ..	9 109	L. S. & M S. ..	Jan. 31 ...	3¼	6	18.225	16.5	19.6	3.1	11.7	5.44	4.74	Fair.	22°	30.17	S. S. E.	12
" " .	"	" " ..	" " ..	"	"	"	37.6	40.1	2.5	21.1	10.84	9.81	"	"	"	"	12
" " .	"	" " ..	" " ..	"	"	"	28.7	28.3	0.4	19.6	6.97	7.12	"	"	"	"	12
Box‡	7 866	N. T. C.	" " ..	3¼	6½	20.205	18.31	14.0	4.31	14.3	0.38	0.51	"	"	"	"	12
"	"	"	" " ..	"	"	"	27.65	28.08	0.43	19.2	3.79	3.64	"	......	....	....	12
"	"	"	" " ..	"	"	"	32.96	42.57	9.61	26.5	12.57	7.93	"	......	....	....	12
Box D	9 312	L. S. & M. S. ..	Feb. 1 ...	3⅛	5½	2.029	18.95	18.17	0.78	12.5	3.93	4.11	"	38°	29.71	S. S. E.	11
" "	"	" " ..	" " ..	"	"	"	27.34	31.51	4.17	20.4	9.31	7.64	"	"	"	"	"
" "	"	" " ..	" " ..	"	"	"	35.22	47.7	12.29	29.1	17.39	11.40	"	"	"	"	"
Stock D ..	5 298	" " ..	Feb. 2 ...	2⅞	5½	2.02	15.0	14.33	0.67	9.4	4 45	4.58	"	10°	30.15	West.	29
" " ..	"	" " ..	" " ..	"	"	"	28.83	32.64	3.81	20.8	11.45	9.95	"	"	"	"	"
" " ..	"	" " ..	" " ..	"	"	"	36.72	46.50	9.82	29.4	22.94	17.51	"	"	"	"	"
" " ..	"	" " ..	" " ..	"	"	"	20.0	17.0	3.0	11.9	3.07	3.77	"	"	"	"	"
" " ..	"	" " ..	" " ..	"	"	"	28.0	30.33	2.33	20.4	10.77	9.90	"	"	"	"	"
" " ..	"	" " ..	" " ..	"	"	"	38.42	47.87	9.45	30.0	24.2	18.96	"	"	"	"	"

Box D....	13 226	" " ..	Feb. 3...	......	......	2.0	14.0	14.42	0.42	9.2	7.6	7.53	Cloudy.	10°	30.23	S. E.	13
" " ...	"	" " ..	" " ..	......	......	....	27.73	34.0	6.27	22.3	13.6	11.13	"	"	"	"	"
" " ...	"	" " ..	" " ..	......	......	....	36.31	47.06	10.75	29.5	22.69	17.02	"	"	"	"	"
Box......	25 684	B. & O	Feb. 10.	$2\frac{13}{16}$ / $3\frac{1}{16}$	$5\frac{1}{2}$ / $5\frac{1}{2}$	19.895	17.75	15.66	2.09	10.4	4.79	5.23	"	"	"	"	"
"	581	M. D. T. Co...	" " ..	......	......	2.016	28.27	35.56	4.29	20.9	7.82	6.16	"	"	"	"	"
"	....	....	....	......	......	....	16.75	14.28	2.47	9.7	5.78	6.27	"	"	"	"	"
"	....	....	....	......	......	....	28.27	32.0	3.73	21.3	11.99	10.55	....	......	......	....	......
"	....	....	....	......	......	....	34.75	46.77	12.02	28.2	20.9	14.64	....	......	......	....	......
WITH TWO EMPTY CARS.																	
Box....	9 109 / 9 312	L. S. & M. S..	Jan. 31...	......	......	28.715	17.75	20.94	3.19	11.6	3.58	2.79	Fair.	22°	30.17	S. S. E.	12
"	"	" " ..	"	......	......	"	29.57	32.50	2.93	21.1	7.83	6.71	"	"	"	"	"
"	"	" " ..	"	......	......	"	24.39	25.84	1.45	17.0	4.41	3.95	"	"	"	"	"
"	"	" " ..	"	......	......	"	35.81	46.92	11.11	28.9	17.33	11.44	"	"	"	"	"
Box D..	9 312 / 3 706	L. S. & M. S...	Feb. 1...	......	......	28.655	10.0	9.49	0.6	6.0	3.68	3.82	... .	38°	29.75	S. S. E.	11
"	"	" " ..	"	......	......	....	21.08	19.11	1.97	13.1	4.48	4.98	....	"	"	"	"
"	"	" " ..	"	......	......	....	26.50	33.23	6.73	20.6	11.66	9.08	....	"	"	"	"
"	"	" " ..	"	......	......	....	36.7	47.2	10.5	29.3	18.43	12.78	....	"	"	"	"
Box....	3 278 / 7 866	N. T. C.......	"	......	......	29.9	16.10	13.27	3.83	8.6	3.69	4.39	....	"	"	"	"
"	"	"	"	......	......	"	26.20	30.60	4.40	19.6	12.05	10.46	....	"	"	"	"

* Tons, 2 000 pounds. † Journals badly worn, from $\frac{1}{16}$ to $\frac{3}{16}$ inch, oiled with petroleum. ‡ From some unexplained cause, the resistance appears to be very low on this car, though everything seemed to be in perfect order about the instrument, brasses worn somewhat, the journals varying in size from wear. § Newly oiled.

WITH TWO EMPTY CARS.—(*Continued.*)

Car.	Number	Railway.	Date.	Journals. Inches. Diameter.	Journals. Inches. Length.	Load including dynagraph car. Tons.	Velocity—feet per second. Initial.	Velocity—feet per second. Final.	Velocity—feet per second. Difference.	Average speed, miles per hour.	Resistance in pounds per ton. Observation corrected for— Level track.	Resistance in pounds per ton. Observation corrected for— Uniform velocity	Weather.	Temperature.	Barometer.	Wind. Direction.	Wind. Velocity, miles per hour.
Box....	3 278 / 7 866	N. T. C.......	Feb. 1	....	....	29.9	22.0	20.30	1.70	14.1	3.40	3.85	....	38°	29.75	S. S. E.	11
"	"	"	"	....	....	"	36.21	45.66	9.45	28.2	17.26	12.32	....	"	"	"	"
Box D..	13 226 / 14 031	L. S. & M. S..	"	....	....	30.2	16.62	15.66	0.96	10.9	5.9	6.1	....	"	"	"	"
"	"	" " ..	"	....	....	"	25.47	29.33	3.86	19.2	10.51	9.16	....	"	"	"	"
" ...	"	" " ..	"	....	....	"	35.87	48.07	12.20	30.0	23.37	16.83	....	"	"	"	"
" ..	5 298 / 6 689	" "	....	....	....	30.315	16.86	17.0	0.14	10.3	5.53	5.50	....	"	"	"	"
"	"	" "	....	....	....	"	22.66	26.40	3.86	16.66	10.32	9.15	....	"	"	"	"
"	"	" "	....	....	....	"	35.0	45.50	10.50	28.2	23.48	18.08	....	10°	30.15	West.	29

WITH THREE AND FOUR EMPTY CARS.

Car.	Number	Railway.	Date.	Journals. Inches. Diameter.	Journals. Inches. Length.	Load including dynagraph car. Tons.	Velocity—feet per second. Initial.	Velocity—feet per second. Final.	Velocity—feet per second. Difference.	Average speed, miles per hour.	Resistance in pounds per ton. Observation corrected for— Level track.	Resistance in pounds per ton. Observation corrected for— Uniform velocity	Weather.	Temperature.	Barometer.	Wind. Direction.	Wind. Velocity, miles per hour.
Box....	9 107 / 9 312 / 3 706	L. S. & M. S..	Jan. 31...	....	....	37.08	18.88	20.37	1.49	12.6	3.82	3.44	Fair.	22°	30.17	S. S. E.	12
"	"	" " ..	" "...	....	....	"	27.79	30.21	2.42	20.3	7.50	6.60	"	"	"	"	"
"	"	" " ..	" "...	....	....	"	35.09	44.36	9.27	27.7	17.17	12.43	....	......	....	....	......
"	"	" " ..	Feb. 1...	....	....	"	17.0	15.7	1.3	10.8	4.15	4.42	Wet Rail.	38°	29.75	S. S. E.	11
"	"	" " ..	" "...	....	....	"	33.78	38.18	4.40	25.3	12.65	10.62	"	"	"	"	"
"	"	" " ..	" "...	....	....	"	36.45	46.50	10.05	30.0	20.30	14.94	"	"	"	"	"

Box*...	3 278 / 7 866 / 2 912	N. T.	" "			39.675	24.55	20.32	4.23	15.0	3.37	4.58	"	"	"	"	"
"	"	" "	" "			"	25.94	30.78	4.84	19.5	9.2	7.45	"	"	"	"	"
"	"	" "	" "			"	32.67	47.33	14.66	27.9	21.97	14.48	"	"	"	"	"
"	"	" "	" "				34.41	42.07	7.66	26.66	16.38	12.64	"	"	"	"	"
Box†...	5 288 / 6 689 / 7 813	L. S. & M. S.	Feb. 2...			3.951	18.8	18.5	0.03	13.3	6.80	6.87	Fair.	10°	*30.15	West.	29
"	"	" "					28.0	31.3	3.3	20.9	12.65	11.40	"	"	"	"	"
"	"	" "				"	36.10	46.50	10.4	29.5	23.43	17.93	"	"	"	"	"
Box....	5 298 / 6 689	" "	Feb. 2..			49.585	20.1	15.3	4.8	11.2	4.87	5.95					
....	7 813	" "	"			"	30.3	34.7	4.4	22.6	19.85	18.03					
Box......	9 198	N. T. C......				"	35.6	48.2	12.6	29.3	24.57	17.82					
WITH SINGLE LOADED CARS.																	
Box D....	13 226	L. S. & M. S.	Feb. 3...			27.35	17.0	18.94	1.94	10.8	6.19	5.76	Cloudy.	10°	30.23	S. E.	13
" " ...	13 226	" "	" "			27.35	28.2	34.0	5.8	21.6	8.50	6.18	"	10°	30.23	"	13
" " ...	13 226	" "	" "			27.35	39.4	48.90	9.5	31.0	19.46	14.06	"	10°	30.23	"	13
Box	3 267	Empire.	" 4...			29.635	17.7	18.1	0.4	12.3	7.26	7.17	Fair.	17°	30.28	West.	20
"	3 267	"	" "			29.635	27.34	31.28	3.94	20.0	11.84	10.40	"	17°	30.28	"	20
"	3 267	"	" "			29.635	35.07	47.33	12.26	28.9	22.14	15.52	"	17°	30.28	"	20
"	661	C. C. C. & I.	" 22...	3	5¼	29.145	14.66	17.97	3.31	9.6	3.31	2.64	Cloudy.	34°	30.11	"	16

* Axles much worn, varying from $2\frac{7}{8} \times 6\frac{1}{2}$ to $3\frac{1}{16} \times 6\frac{1}{2}$ inches, brasses 6 inches long. † Axles and wheels much worn, axles varying from $2\frac{7}{8} \times 5\frac{1}{2}$ to $3\frac{1}{16} \times 5\frac{1}{2}$ inches.

WITH SINGLE LOADED CARS.—(*Continued*).

Car.	Number	Railway.	Date.	Journals. Inches. Diameter.	Journals. Inches. Length.	Load including dynagraph car. Tons.	Velocity—feet per second. Initial.	Velocity—feet per second. Final.	Velocity—feet per second. Difference.	Average speed, miles per hour.	Resistance in pounds per ton. Observation corrected for— Level track.	Resistance in pounds per ton. Observation corrected for— Uniform velocity	Weather.	Temperature.	Barometer.	Wind. Direction.	Wind. Velocity, miles per hour.
Box......	661	C. C. C. & I...	Feb. 22..	3	$5\frac{3}{4}$	29.045	13.73	14.13	0.40	8.4	1.35	1.28	Cloudy	34°	30.11	West	16
"	661	" " "	" " ..	3	$5\frac{3}{4}$	29.045	25.09	31.42	6.33	19.6	7.35	5.13	"	34°	30.11	"	16
"	661	" " "	" " ..	3	$5\frac{1}{4}$	29.045	29.73	36.0	6.27	22.9	6.96	4.39	"	34°	30.11	"	16
"	5 635	S. & J. S. E.	" 23...	$2\frac{15}{16}$	$5\frac{1}{2}$	28.76	16.27	17.33	10.6	11.2	9.49	9.27	Clear.	5°	30.30	N. N. W.	20
"	5 635	" " "	" " ..	$2\frac{15}{16}$	$5\frac{1}{2}$	28.76	17.2	16.75	0.45	11.2	7.12	7.21	"	5°	30.30	"	20
"	5 635	" " "	" " ..	$2\frac{15}{16}$	$5\frac{1}{2}$	28.76	27.6	30.12	2.52	19.7	11.88	10.98	"	5°	30.30	"	20
"	5 635	" " "	" " ..	$2\frac{15}{16}$	$5\frac{1}{2}$	28.76	27.07	33.33	6.26	20.6	15.47	13.10	"	5°	30.30	"	20
"	2 596	N. Y. C.	" " ..	$3\frac{1}{4}$	$5\frac{1}{2}$	31.17	15.73	17.47	1.74	10.3	9.33	8.98	"	5°	30.30	"	20
"	2 596	" " "	" " ..	$3\frac{1}{4}$	$5\frac{1}{2}$	31.17	17.33	18.30	0.97	11.3	6.36	6.14	"	5°	30.30	"	20
"	2 596	" " "	" " ..	$3\frac{1}{4}$	$5\frac{1}{2}$	31.17	28.80	33.33	4.53	21.6	11.63	9.88	"	5°	30.30	"	20
"	2 596	" " "	" " ..	$3\frac{1}{4}$	$5\frac{1}{2}$	31.17	27.47	32.80	4.33	20.9	11.2	9.20	"	5°	30.30	"	20
"	8 293	L. S. & M. S.	" " ..	$3\frac{1}{4}$	$5\frac{1}{2}$	29.1	15.33	21.64	6.31	10.7	10.75	9.30	"	5°	30.30	"	20
"	8 293	" "	" " ..	$3\frac{1}{4}$	$5\frac{1}{2}$	29.1	17.2	14.8	2.4	10.1	6.16	6.63	"	5°	30.30	"	20
"	8 293	" "	" " ..	$3\frac{1}{4}$	$5\frac{1}{2}$	29.1	28.96	38.80	9.84	23.40	16.82	12.66	"	5°	30.30	"	20
"	8 293	" "	" " ..	$3\frac{1}{4}$	$5\frac{1}{2}$	29.1	25.55	31.0	5.45	19.75	10.61	8.69	"	5°	30.30	"	20
"	982	M. D. T.	" 24...	$3\frac{5}{16}$	6	33.19	12.39	10.45	1.94	6.9	10.29	10.56	Snow.	13°	30.34	West.	9
"	982	" " "	" " ..	$3\frac{5}{16}$	6	33 19	17.64	17.61	0.03	10.75	7.90	7.90	"	13°	30.34	"	9

"	982	" " "	" " ..	$3\frac{5}{16}$	6	33.19	29.09	34.10	5.01	21.18	12.89	10.93	"	13°	30.34	"	9
"	982	" " "	" " ..	$3\frac{5}{16}$	6	33.19	26.27	25.43	0.84	17.5	7.70	7.96	"	13°	30.34	"	9
"	856	C. C. C. & I.	" " ..	$3\frac{5}{16}$	6	29.845	18.40	16.53	1.87	12.0	8.74	9.14	"	13°	30.34	"	9
"	856	" " "	" " ..	$3\frac{5}{16}$	6	29.845	18.27	18.09	0.18	12.8	8.78	8.82	"	13°	30.34	"	9
"	856	" " "	" " ..	$3\frac{5}{16}$	6	29.845	25.02	31.60	6.58	19.5	9.03	6.72	"	13°	30.34	"	9
"	856	" " "	" " ..	$3\frac{5}{16}$	6	29.845	27.42	33.73	6.31	21.1	9.04	6.65	"	13°	30.34	"	9
"	8 082	L. S. & M.	" " ..	$3\frac{1}{8}$	$5\frac{1}{2}$	30.845	15.17	11.77	3.40	7.75	6.61	7.17	"	13°	30.34	"	9
"	8 082	" "	" " ..	$3\frac{1}{8}$	$5\frac{1}{2}$	30.845	22.71	20.33	2.38	14.1	6.95	7.58	"	13°	30.34	"	9
"	8 082	" "	" " ..	$3\frac{1}{8}$	$5\frac{1}{2}$	30.845	27.8	33.7	5.9	21.6	15.25	13.03	"	13°	30.34	"	9
"	8 082	" "	" " ..	$3\frac{1}{8}$	$5\frac{1}{2}$	30.845	25.68	23.17	2.51	17.0	6.93	7.68	"	13°	30.34	"	9
"	2 052	I. B. & W. W.	" " ..	$3\frac{1}{8}$	7	31.05	18.0	17.20	0.8	11.85	5.74	5.91	"	13°	30.34	"	9
"	2 052	" " " "	" " ..	$3\frac{1}{8}$	7	31.05	16.0	15.47	0.73	10.60	4.99	5.09	"	13°	30.34	"	9
"	2 052	" " " "	" " ..	$3\frac{1}{8}$	7	31.05	26.94	24.67	2.27	17.1	5.11	5.84	"	13°	30.34	"	9
"	2 052	" " " "	" " ..	$3\frac{1}{8}$	7	31.05	24.44	21.78	2.66	15.7	4.89	5.65	"	13°	30.34	"	9
"	7 070	Empire.	" 25 ..	$3\frac{1}{8}$	$5\frac{1}{2}$	30.60	18.62	14.2	4.42	10.9	3.92	4.82	Clear.	17°	30.11	S. S. E.	12
"	7 070	"	" " ..	$3\frac{1}{8}$	$5\frac{1}{2}$	30.60	19.0	14.93	4.07	11.3	2.77	3.62	"	17°	30.11	"	12
......	7 070	"	" " ..	$3\frac{1}{8}$	$5\frac{1}{2}$	30.60	25.78	28.17	2.39	18.6	6.69	5.90	"	17°	30.11	"	12
......	7 070	"	" " ..	$3\frac{1}{8}$	$5\frac{1}{2}$	30.60	24.03	29.2	5.17	18.5	8.08	6.44	"	17°	30.11	"	12
"	7 070	"	" " ..	$3\frac{1}{8}$	$5\frac{1}{2}$	30.60	18.17	13.90	4.27	10.2	2.68	3.51	"	17°	30.11	"	12
"	7 070	"	" " ..	$3\frac{1}{8}$	$5\frac{1}{2}$	30.60	17.37	9.8	7.57	7.9	3.40	2.66	"	17°	30.11	"	12
"	7 070	"	" " ..	$3\frac{1}{8}$	$5\frac{1}{2}$	30.60	23.86	23.90	0.04	16.55	4.90	4.90	"	17°	30.11	"	12
"	7 070	"	" " ..	$3\frac{1}{8}$	$5\frac{1}{2}$	30.60	29.33	20.48	8.85	17.1	2.44	5.18	"	17°	30.11	"	12

ONE AND TWO LOADED CARS.

Car.	Number	Railway.	Date.	Journals. Inches. Diameter.	Journals. Inches. Length.	Load including dynagraph car. Tons.	Velocity—feet per second. Initial.	Velocity—feet per second. Final.	Velocity—feet per second. Difference.	Average speed, miles per hour.	Resistance in pounds per ton. Observation correct for. Level track.	Resistance in pounds per ton. Observation correct for. Uniform velocity	Weather.	Temperature.	Barometer.	Wind. Direction.	Wind. Velocity, miles per hour.
Box......	1 475	B. & A.........	Feb. 25...	......	......	27.61	16.61	15.24	1.37	9.5	3.79	4.06	Clear.	17°	30.11	S. S. W.	12
"	"	"	"	......	......	"	20.87	16.17	4.70	13.2	1.98	3.06	"	"	"	"	"
"	"	"	"	......	......	"	24.91	26.17	1.26	18.0	4.2	3.80	"	"	"	"	"
....	....	....	....	......	......	"	26.0	27.12	1.12	18.5	3.62	3.25	"	"	"	"	"
....	....	....	....	......	......	"	17.64	17.19	0.45	11.6	2.76	2.86	"	"	"	"	"
....	....	....	....	......	......	....	24.89	30.60	5.71	18.6	5.98	4.01	"	"	"	"	"
Coal D*	13 226 / 14 031	L. S. & M. S..	Feb. 3....	......	......	45.35	18.41	16.1	2.3	10.8	4.75	5.24	Cloudy.	10°	30.33	S. E.	13
"	"	" "	"	......	......	"	27.6	34.4	6.8	21.8	14.62	12.0	"	"	"	"	"
......	....	....	....	......	......	....	35.53	49.30	13.77	28.6	22.11	14.83	"	"	"	"	"
Box....	3 277 / 3 744	Empire	Feb. 4....	......	......	52.55	15.5	17.0	1.5	8.7	5.94	5.64	Fair.	17	30.28	West.	20
......	....	....	....	......	......	....	27.12	32.82	5.70	29.9	9.79	7.69	"	"	"	"	"
Box.....	5 635	S. & I. R. E...	....	......	......	....	33.78	46.0	12.22	28.2	19.12	13.10	"	"	"	"	"
......	8 293	L. S. & M. S...	Feb. 23...	$2\frac{15}{16} \times 6$ / $3\frac{1}{2} \times 6$		48.06	23.69	27.46	3.77	17.8	11.77	10.57	Clear.	5°	30.39	N. N. W.	20

* Newly oiled.

AMERICAN SOCIETY OF CIVIL ENGINEERS.

INCORPORATED 1852.

TRANSACTIONS.

NOTE.—This Society is not responsible, as a body, for the facts and opinions advanced in any of its publications.

DISCUSSIONS OF SUBJECTS PRESENTED AT THE EIGHTH ANNUAL CONVENTION.*

ON THE METRIC SYSTEM OF WEIGHTS AND MEASURES.†

MR. CLEMENS HERSCHEL.—The question now before the Society, naturally resolves itself into two parts: is it the sense of the Society that the metric system of weights and measures shall be introduced in the United States, ever, or at any time; and, in case the desirability of such a change is affirmed, then, what steps shall be taken by the Society to further this object? I submit that it is time this Society takes definite action upon the question; and, if necessary, continue the discussion upon it from meeting to meeting, but do not lay the matter upon the table again without a decided expression of opinion.

The subject is now well before the public; at the present session of Congress, some twenty petitions in favor of the introduction of the metric system of weights and measures were presented: from the Massachusetts Legislature, from Yale College, the Institute of Technology, the Boston Society of Civil Engineers, the Civil Engineers' Club of St. Louis, Mo., &c.; the system is advocated in professional periodicals, in Scribner's Monthly, &c.; we have the American Metrological Society especially organized to push this and other reforms on measures, and the United States Government has distributed accurate metre bars among the several States. Civil engineers, who make it part of their business to measure quantities for other people, are bound to know whether or not this is a good thing. If it is, then the American Society of Civil Engineers ought to support it, actively and energetically; if, on the other hand, it is not a good thing, and, as some think, a few foolish enthusiasts are trying to crowd an objectionable thing upon an ignorant and confiding community, then let the Society appear as the protector of this commu-

* Continued from page 354. † Referring to communications from Clemens Herschel, Proceedings, Vol. I, page 321; Vol. II, page 61.

nity and stamp the thing out if it can. Although the Society may be utterly unable to prevent the final adoption of the metric system, it can obstruct, and at least, by condemning the whole thing, can prevent its debate at the meetings; whereas, laying it on the table, leaves an ever present opportunity for it to be taken up again.

I foresee, however, that the Society is not going to condemn, totally and unequivocally, the metric system; the notion of having all measures decimally arranged has already too strong a hold upon the people, too many members have in various ways expressed their approval of it, and the question still remains as to what steps it is best to take for the gradual and easy introduction of the system in the United States. With these views, I offer the following resolutions, to be voted on separately:

Resolved: That the American Society of Civil Engineers will further, by all legitimate means, the adoption of the metric standards in the office of Weights and Measures, at Washington, as the sole authorized standards of weights and measures in the United States;

Resolved: That the Chair appoint a committee of five, to report to this Convention a form of memorial to Congress, in furtherance of the object expressed.*

While I trust that this Society will, by a large majority, if not unanimously, pass these resolutions, I have no desire to appear in ignorance of the objections that have been brought against similar resolutions, many of which may also be brought against these now offered. "In all the transactions of life," says an anonymous author, "we are called upon to strike a balance between the advantages and the disadvantages of a proposed mode of action. Use enables all to see both sides of the account; but in anticipation, the great majority is apt to see only the disadvantages." Here are some of the objections that have been brought against the metric system:

1°. The metre is not an accurate measure, not the millionth part of the quadrant of the earth; but no measure is or can precisely be; it is not in the nature of things; it is now, by many copies of standard platinum metre bars, as firmly and accurately determined as any measure well can be.

2°. It is of foreign or French origin. Not so; many nations took part in its establishment.†

* This resolution was subsequently discussed: for record of action taken, see Proceedings, Vol. II, page 85. † Two commissioners from Holland, two from Spain, and one each from Sardinia, Denmark, Tuscany, Rome, Cis Alpine, Ligaria, Switzerland and Piedmont.

3°. The names are French. Not so; Greek and Latin names were selected purposely to avoid this objection.*

4°. There is too much difference between the old and the proposed new measures; this is an advantage in effecting the change; it will be one thing or the other.†

5°. The metre is too large. It is just as easy to argue that the foot is too small; we reckon earth-work in cubic yards, and even that has been deemed too small, and the "square" is the result. Mariners talk about fathoms in depth and fathoms of rope. Cloth is measured by the yard; rifles have a range of so many yards; all this, because the foot is too small. We speak of 20-inch cylinders, barometer 30 inches, &c., because a 12-inch unit is too small; in handling the foot measure, most every one has experienced the nuisance of a foot-rule, and has settled on a 2-foot rule instead, the same as has every mechanic in the land, because the foot is too small. Now what is the truth in this matter? Why, it is simply, that no one unit is the correct thing for all purposes. We need many units of measure; in the workshop the centimetre, or the millimetre; for bridge spans, earth-work, &c., the metre; on the road the kilometre, and so on; but it is of the greatest advantage to have all of these units intimately related to one another, and to be able to convert the one into the other by a mere shifting of the decimal point; and when thus constituted, these different units are in effect only one and the same unit of measure, and one that is applicable to all purposes.

6°. Once in a while we hear an objection like this: "that the foot is the length of a man's foot, the inch the length of the first joint of a man's middle finger; the yard the length of his leg, &c.; so every man has constantly with him a scale of measurement that will give a tolerable approximation to the standard dimensions."‡ I hardly know whether this is seriously meant as an objection or not; I have read, and could have copied (as I have copied these of English linear measures) analogies expressed in the metric lengths; but to what useful end? They evidently have not the slightest practical value. As a comment on the crudeness of such views, it might be pointed out, that although, as quoted, the length of a leg is a yard long, history I believe, teaches us that it was the length of a certain English king's arm that was originally taken as a yard; so

* Spain says, *metro*, *litro*, &c. Similarly, English speaking nations would anglicize the metric terms.

† These were heads noted for the speaker's use; they do not adequately represent the remarks actually made, which the speaker has had no opportunity to write out.—C. H.

‡ But one shoe in 1 000 is a foot long. The average length of a man's foot is given by authorities at 25.5 centimetres $= 10\frac{1}{16}$ inches, while one foot $= 12$ inches, is 30.5 centimetres; and what is called a foot ranges from 25 centimetres in Hessia, to 51.37 in Piedmont.

that this form of comparison does not distinguish the difference between a leg and an arm, and, in following it, it will be impossible to tell whether one is standing on his feet, or on his head. However, it is well enough for every one to carry with him, some approximate measures of the sort indicated. I use my span ; it is about 9 inches, or 23 centimetres long, and it is as easy to remember the one as the other.

7°. The great objection most frequently brought to bear against the proposed, or any change, is however, the trouble and expense that it is anticipated will be caused in making the change. Well, is there any good thing to be had in this world without hard work ? Nevertheless, the good things are believed to be worth what they cost. I think it is generally admitted that the change must come some time ; then why not now ? We shall never be in any better position to make it ; there will always be the same reference to trouble and expense there is now, and perhaps we could make the change at even less expense now, than ever again. There will always be some difficulty, though I think it will be far less than is anticipated. The German nation, between 1868 and 1872, made the change ; by setting the date of the final adoption of the metric system, as the sole standard, four years in advance, the people found that during those years most of the change had been quietly effected, so that when the dreaded day came, there remained hardly anything to be done. It is too often forgotten, that already in our own history, one change such as contemplated, has been made and without great difficulties. Doubtless it was annoying, and caused the conservative portion of the community great trouble to change from the shilling, the ninepence and the four bit ha'penny, to the decimal coinage now used. As regards the trouble to be caused to our own profession, I apprehend that there are many engineers present who can speak of it from personal experience, having been employed, at one time or another, in foreign countries. As for myself, I have made the change, from one system of weights and measures to another, twice in the course of my life.* In fact, the figures that one carries in his head, at any one time, are after all, pretty light baggage. The bulk of knowledge must always stay in the books, and these are as readily printed in the one measures as the others. Expe-

* I graduated at the engineering school connected with Harvard University, and learned there only the English measures ; I then studied $3\frac{1}{2}$ years in France and Germany, studying only in the metric measures ; then returned to practise in the New England States, and resumed the English measures. I should exaggerate, were I to speak of any serious inconvenience caused by these changes. The figures carried in one's head are few, and it is as easy to remember 7 kilograms to the square centimetre, as it is 10 000 pounds to the square inch.

rience has shown in England and Germany, that the mechanic and laborer very easily learn the new measures; in Germany, the change was often effected in a single morning without interrupting work in hand, the only thing necessary being to give the men new drawings figured in metric measures, with new rules, and to take away the old ones.

An especial fear is expressed as to the expense of changing in machine shops, and with some reason. In the case of any reform, it is always some classes who are unfavorably affected; however, the progress of the world is not stopped on that account, and looking the matter in the face, what does it really amount to? If the public obliges the machine shops to change taps, dies, gauges, &c., let the machine shops oblige the public to pay for the change whenever they sell their goods. The change will cause expense to the shops, and it will bring them work; in the long run, which will be ahead—the machine shops or the public? Or, if all the machine shops will need new taps and dies, will not the tap and die-makers have a good run of work? And is it not the grand final result of any change that more or less money has changed hands, smart men getting the most? The actual loss of property will be so distributed during the progress of the change, that it is scarcely felt.

There are some reasons for supposing that the difficulty and cost in the machine shops have been overestimated. We have had in this country, as opposed to the introduction of the metric measures, a strong statement of these anticipated troubles, by Mr. Coleman Sellers.* Also in the *Engineering News*, of May 13, the chief engineer of a well-known bridge company in the West, has written on the subject; he says, speaking of the state of affairs after the change has been effected: "A pulley must be replaced on a 3-inch shaft, it must be bored out to 7.620 402 3+ centimetres; the man at the drill would stand aghast." I fancy the superintendent of the shop could prevent such a startling exhibit, however, in three ways: he could give the man the dimensions of the work to be done, in millimetres and tenths of millimetres, if need be, and thus drop five out of the seven decimals. Anything finer than a tenth of a millimetre (= 0.004 inch) is beyond the powers of any ordinary machine tool, and to read to all the decimals above quoted would require a finer microscope than has yet been constructed. Again, he could show the man the shaft, and have him fit his pulley on *that* shaft, or, in repairing old work, and until all the old work has disappeared, the workman

* On two occasions: in an address before the American Railway Master Mechanics' Association, and in a recent majority Report to the Franklin Institute of Pennsylvania.

could use the old foot rule.* There will always be a way to get over these seeming difficulties, and if we were all called on to-day to compete, say for government work, described in metric dimensions, we would readily find the way to do it.†

To show there is no practically insuperable difficulties in making the change (and that is all that is claimed, for all admit that some work is required to achieve anything), I may mention three cases—one in England, and two in this country—of workshops which already use the metric measures. The one in England, a machine shop, making many cog-wheels among other things, is mentioned in "Minutes of the Institution of Civil Engineers," 1868-9, page 589. The case nearer home, is that of the American Watch Co., at Waltham, Mass. The works were started in 1857, and the change of measures made in 1869. Mr. A. Webster‡ originally adopted the principle that the finer the measures used by the mechanic, the better work he will produce. The hundredth of an inch was, however, too coarse for his purposes, and the thousandth too fine to be seen with the naked eye; the tenth of a millimetre was just about right. This was his starting point, but in the course of his investigations he soon saw that the metric measures would enable him to construct measuring tools, gauges, &c., of an accuracy and convenience in use that could not be obtained by means of the tools and measures first placed in the factory. He hesitated for two or three years in the matter, studied the subject carefully, and finally concluded to change all the taps, dies and gauges, and from a certain day, when all the new tools were ready, to use none of the old tools and measures, except in the repair of old machines. As I have already stated, he does not regret the step. He has found no difficulty in taking workmen as they come, and making them work according to the new measures.

Waltham watches sell, and the factory prospers. The agent of the American Watch Tool Co., Mr. J. Z. Whitcomb, was four or five years in the metric shop, and when he started his own works, he began them on the metric system. It must be remembered, in speaking of these cases, that

* This last is the method adopted in the Waltham Watch Factory, of which I shall speak later. In about four years, they used the old taps and dies not over once a month, and now hardly ever.

† In the paper from which I have quoted, is a letter from Genl. Franklin, of Colt's Patent Fire Arms Manufacturing Co., of Hartford, Conn., in which he says that these works stand ready to-day, to make guns according to the metric dimensions, at the same rates, as according to the English measures; and this, I suppose, is equally true about the bell-punches, and the Baxter Steam Engines, with which these works bless the community.

‡ The Superintendent of the machine shop connected with the works, at that time, and now Assistant General Superintendent of the factory.

of course the labor and expense of making the change was far greater for these factories, acting alone by themselves, than it would be for one acting in common with the whole community. These works are also subjected to annoyances in making the change, which would entirely disappear if the rest of the community would change with them; but in spite of all this, they have done single-handed what is now proposed all should do in common. Their example seems to indicate that the difficulties to be overcome in making the change have been overrated.

Another instructive instance is that of the whole German nation, which, as before stated, changed its measures to those of the metric system, between 1868 and 1872. Let one refer to files of German technical journals, and he will find the same classes, genera and species of arguments, pro and con, as we now see in our journals and hear in our societies; the same difficulties were anticipated, the same items of expense and trouble were urged against the adoption of the new measures, and yet the change was made, easily, without apparent trouble, and Germany would not go back to the old measures any more than we would return to the pounds, shillings and pence of our forefathers. The example is extremely instructive, and shows that the difficulty, the expense of making the change, is only too often sadly overrated. "In anticipation, the great majority sees only the disadvantages of any proposed change. Use enables all parties to see both sides of the account."

In thus speaking of the objections that have been brought against the introduction of the metric system in the United States, I have incidentally alluded to some points in its favor; but overlooking for the present some minor recommendations, the system has these three great advantages:

First.—It is decimal. Now, will it be necessary for me to argue before this assembly in favor of the advantages of this, or to reply to those who are never tired of attempting to prove that our whole science of arithmetic is at fault, and should have been based on the duodecimal scale? I fancy not. I am addressing the citizens of a country possessing, as do most of the nations of the present day, a decimal coinage—a reform started, however, I believe, by ourselves. I am speaking to members of a profession who have introduced the decimal division of the foot in their practice. Until our arithmetic is changed, possibly ten centennials from now, a decimal division of our

measures is the best one. But it is one of the advantages of the decimal division that it retains a binary form of division up to halves and quarters, as for instance, halves and quarters of a dollar and beyond this point, into the labyrinth of eighths, sixteenths, thirty-seconds, sixty-fourths, &c., this binary form of division more than *ceases* to be a virtue.

Second.—A great advantage of the metric system is, that it has units of capacity and of weight which bear a simple relation to the unit of length, and have analogous names. This renders it a more useful servant in all the daily affairs of life, and makes it much easier to teach to children. It has been urged, by experienced educators, that they could devote one whole year of every child's school time to other and useful objects, if the metric system of weights and measures were in use, instead of that, upon which much of their time is now wasted. One whole year's labor of every child to be educated, many years' time in all, of every teacher throughout the land, and all this to keep alive a deformity, a misshapen organization that is the offspring of centuries of chance and accident, and of the edict of long lines of stupid sovereigns of the past. This it is proposed to supplant by a clear system of measures, that from the beginning was designed to be simple;—and here I touch upon its other great advantages.

Third.—It was designed to be international, the common property of all mankind. I will leave to abler speakers to portray the beauty of this: the idea of all nations having one unit of measure, and the advance toward a good understanding of the people of the earth among themselves, when this shall once have been accomplished. Such thoughts will suggest themselves to all, in the presence of the Exhibition which we have come to see. But let us look at the matter in the same spirit of calm inquiry and of cost, that I have endeavored to keep before me. Here is a system of weights and measures, not *French*, as many would have you believe, but already adopted among so many nations, that it is far easier to name those which have *not* adopted the metric measures, than those which *have;* and in *all* of them which yet lack this conformity to a desired end, there is, and for years has been, an endeavor to bring about this "keeping step to the music of the Union." Is there not great probability that in the end *all* the nations of the earth will be agreed on this point? *

* We hear quoted, extracts from John Quincy Adams of 1821, and references to the yard, the metre and the pendulum of Sir John Herschel. Whatever may have been the opinion of these great men in the past, it is evident to us, it would now be evident to them, that only by adopting the metric system, can uniformity among nations be ever expected at this date.

In fact, the decision depends now, finally and irrevocably, upon *either* the United States or upon England. Let one adopt the metric standard, and all that yet remain to change (they are only Great Britain, Russia, the United States, and some minor and Oriental nations) will undoubtedly join the majority. Which is better, that we follow the lead of England in this matter, or that the English shall find it necessary to follow us; and follow they must, if we adopt the metric system of weights and measures, for by so doing we bid strongly for much of the import trade of other nations, and we make a strong effort for largely increasing the export trade of the United States at the expense of England. Which will nurture the nation most, to still run in an old rut, or to increase our export trade, by accommodating our customers? Is not this trade one great element of the weath of a nation? Now, here are the great South American countries—Brazil, Peru, Chili, &c.—large buyers, near neighbors, and all of them have the metric system of weights and measures. Does not every builder of machinery know, that before the inhabitant of these countries will buy a machine of us he will think twice about it, because the repair of that machine in his own country, constructed as it is with a different scale of lengths, will be just a trifle annoying to him? And yet we lead the world in the invention of labor-saving machinery. This is the precise kind of machinery that is most in demand in many countries, which manufacture little and buy much; and for the sake of an ounce of trouble spread over four or five years of a lazy man's life, we are to make no attempt to accommodate these customers, or effort to increase our export trade to foreign countries, even though it be at the expense, perhaps, of our great rival in the sale of machinery to the world. I trust not, and what I have argued in the case of machinery is more or less applicable to all the manufactured products of the nation; the export trade in all of which, could be notably increased, I submit, by being manufactured to suit the wants of metric buyers. These are our customers; England buys of us raw products; she *sells* us manufactured goods; she sells the same to other nations. If we wish to gain an advantage over her, we must please her customers better than she does.

The advantages of the metric system, it is true, are few and can be briefly stated. To those who are familiar with its merits, they are so patent, that it is even difficult for them to present these advantages in a formal manner. The disadvantages, fancied and real, that have at times been brought up against the introduction of the metric system, it is

true, are numerous ; but they are often trivial, and the light of analogous experience teaches us that whatever they are, they fade into insignificance, when by a combined movement, all classes of a community strive to have and enjoy the resultant benefits.

Upon the subject matter that is brought up by the second resolution which I have offered, it will not be necessary to say much at this time. Its discussion would come more properly upon the presentation of the report of the committee, which the resolution contemplates. It may not be improper to recall, however, that this is a free country. I am not disposed to "let the eagle scream" to that extent as to argue that in this country, or in republics *alone*, it is the voice of the people that leads to legislation, or brings about a needed reform in the affairs of state ; but it is, at all events, a marked feature in the administration of our affairs, that all reforms must come in obedience to the voice of the majority. Then so much the more, it is the duty of every citizen of the Republic, energetically to work for reform, to support it openly, combatting error, educating where he may, his neighbor. A body so large, so widely distributed, of so high a degree of general education as the American Society of Civil Engineers cannot fail to exercise a marked influence upon the affairs of the nation, if it will. With these views I have brought the subject before you. I trust that the Society will speak with no uncertain voice.

Mr. Joseph B. Davis.—An objection to the introduction of the metric system has arisen in the West, which has much weight in that part of the country. The land there is all surveyed in miles, squares or sections, and then subdivided. It was said that the change might cause great difficulty in making these subdivisions. But I think a little computation will show that this objection may be readily overcome ; also that in this case, as in others, the metric system will simplify the work. The mile measure—which according to our standard is a few metres more than 1 600 – can be easily made to conform to it: it then will represent a distance much nearer a true mile than the sides of the sections are found to be, when retraced, and which readily divides into halves, quarters, eighths and so on to the smallest subdivision.

Mr. Coleman Sellers.—As my name has been used in this discussion, I think it proper to say a few words on this important subject. Without going so far from here as to the Waltham watch factory, if Mr. Herschel had visited our shops in this city*, he would have seen, that in

* William Sellers & Co., 1600 Hamilton Street, Philadelphia.

one of the most important departments, the metric system has been used for more than sixteen years. It is as easy for any one of our workmen to use the metre as it is for him to use the inch, so far as the mere measurement of any object is concerned; but the inch as a unit of measure in the machine shop has advantages over any of the subdivisions of the metre. Such subdivision gives the decimetre, too long a unit, the centimetre too short a unit, for convenience in mental use. The inch, either decimally divided, or divided into halves, quarters, eighths, &c., is a very much more convenient unit. The scientist finds the French system of great use in its harmony, and as it presents certain advantages in calculation, he naturally urges its universal adoption. For my own part, long use has made me familiar with it, and in the drawing room it is as easy to apply as any other scale of equal parts.

Those who advocate our adoption of the new system to the exclusion of the old one, would do well to consider the cost of the change; this matter of cost is of more serious import than all else, and is not always clearly seen by the advocates of the change. In the preparation of the paper read by me before the American Railway Master Mechanics' Association,* I took the trouble to make a careful calculation of the cost of a radical change from one system to the other. This calculation was based on the assumption that in the new unit of measure, long decimals could only be avoided by altering sizes into the nearest dimensions in millimetres; hence that the standards of measures, the inch taps, the inch gauges, the inch reamers, mandrils and drills must be replaced by sizes in even millimetres. In a machine shop such as our own, employing 600 hands, this change would cost $150 000; which is *rather* a heavy tax when the advantages to be gained are not such as would be felt or appreciated by us or by our workmen, but would merely force a conformity with other nations who have seen fit to adopt the metric system.

The true value of the inch, as the unit of measurement in the machine shop, can only be fully appreciated by those who have for many years been familiar with both systems. When we introduced a French invention—the injector for feeding steam boilers with water—the sizes of the instrument were given in millimetres; thus a No. *four*, injector, was one in which the delivery was 4 millimetres in diameter at its smallest place. To have retained this size and expressed its dimensions in fractions of an inch would have given to us the same truth, as would a change of name to our inch sizes and the expression of these same dimensions as in the French measure. So we adopted the metre

* "The Metric System in our Workshops."

as the unit of measure in this case, and have seen no reason to regret having done so. But its continued use during many years has only confirmed us in the advantage of the inch, a unit which can be divided into any convenient subdivision at the option of the user. The Waltham Watch Factory, or any other manufactory using only short measurements, will find the metric system as convenient as we have found it in its use in building injectors, but for those shops that deal in larger sizes of parts the inch is very much more convenient.

Let any other unit of measurement be introduced, we could not abandon our nomenclature of the inch; for there is an immense stock of material, sized by inches that must be continued, to avoid confusion. Thus, all the gear wheels in the land are spaced by inches and the vulgar fractions of the inch, in the teeth; which if of $\frac{1}{2}$ inch, of $\frac{3}{4}$ inch, of 1 inch or of 6 inches pitch, we must continue to use, for they are in use, and the existing patterns number by thousands; and which pitched by the inch in spacing their teeth, make a decimal division of the inch in placing their centres. So to avoid this, a peculiarly American system was adopted, called the *per-inch* system; this was introduced, I think, by the makers of cotton and woollen machinery in New England. By this system the wheels are placed with their centres at even inches apart, and their teeth are spaced by decimal divisions of the inch. A wheel 10 inches in diameter, 10-*per-inch*, has 100 teeth in its circumference; one 10 inches diameter, 3-*per-inch*, has 30 teeth, and one 10 inches diameter, 1-*per-inch*, has 10 teeth only. This beautiful system can only be perpetuated by the perpetuation of the inch as the unit; that it will be continued in spite of any edicts to the contrary, is quite apparent to the users of the system. Now that many of the countries of the world have introduced the metre into their workshops, it is instructive to note how few have ventured to express their screw threads by any nomenclature save that of so many threads to the inch. When the German Government adopted the metric system it did a wise thing; it had a country made up of many small states, all using different units of measurement; Alsace and Lorraine were metric using provinces. It is therefore safe to infer that political reasons influenced the adoption of the French system.

For some years past, I have been intimately acquainted with Prof. Julius E. Hilgard; to him is largely due the fact that the French system is legal in America; he took an active part in having it legalized. If you or I use the metric system, we are doing a lawful thing, and no one can object if we sell our goods by the metre, if we wish to do so. Prof. Hilgard, however, thinks with me that just here the matter should end,

and that any compulsory law would be highly injurious and should not be contemplated now. If the metre is better than the unit, if the French system presents advantages which render its adoption a matter of convenience or profit, let its adoption be voluntary not forced. Show the people that they will be the gainers by the change, and they will make it. At this time it is impossible to influence Congress in the direction of a change--the French system is legal, and yet in but one department of Government is it used. Prof. Hilgard can tell you that the Coast Survey department uses the metre as its unit of measurement on the surface of the earth, and yet in this department the foot divided decimally is used for all vertical measurement. The United States Survey, apart from the Coast Survey, uses the foot and the mile; every part of our country is divided into feet and recorded in feet, and is bought and sold by some measurement expressed in feet, or some measure involving feet.

It is not only in the machine shop that the inch is so convenient. When panes of glass are cut (and they have for years been cut to sizes expressed by inches), the nomenclature of their sizes 8 × 10, 10 × 12, &c., is beautiful in its simplicity. A carpenter, in laying out a window frame for four lights inside, of 10 × 12 glass, can make a mental calculation involving few figures: thus 2 frames 2 inches each, 4 glasses 10 inches each, 3 mountings of ¼ inch each, will give him on his fingers, a frame 44¾ inches wide, with but little mental effort. Let the advocates of the French system desire a simpler rule than now attains with the carpenter, ere they seek to make the change compulsory.

Mr. Julius E. Hilgard.—The discussion of this subject has a special interest for me, as the matter of standards of measurement is under my charge at Washington. That I am disposed to forward any feasible measure looking towards unification of standards, which is now possible only by the adoption of the metric system in this country, I will be credited with, when it is remembered that I am one of those who, ten years ago, were instrumental in procuring the legalization of the metric system in the United States. I beg, therefore, not to be understood as unfriendly to the proposition of the Boston Society of Civil Engineers when I express my opinion that it is unadvisable to urge additional legislation upon this subject, for I think Congress has done all it is willing to do for some time in that direction; and I fear that if we press the matter too hard, the measure will receive so great a set back as to make us all regret that it was brought forward. It is with the greatest difficulty that I can get a hearing at all upon the subject, before Congress. From New York, Chicago, St. Louis, Philadelphia, and other cities, from all the leading

universities and colleges, petitions and memorials have been sent in favor of our participation in an International Bureau of Weights and Measures, and, as yet, without availing anything;—the proposition lies dormant in a committee room, certain to be rejected if called up. I know most of the members in Congress, and do not think it can be passed for some time yet, and certainly not at the present session.

It would, in my view, be perfectly useless to urge the adoption of any legislation looking towards making the use of the metric system obligatory. I had a conversation a few weeks ago with a member of considerable influence, whose name you are all familiar with, and towards whom now all eyes are fixed, who said he did not think that the metric system would ever be introduced in the United States, nor did it appear to him at all desirable. Such appears to be the situation at Washington. Meantime, gentlemen, you can use the system; as far as you are able, you might have it taught in the schools, and teach the rising generation its advantages, and when they are seated in the Houses of Congress, we will press it. I am speaking to the friends of the measure, and if they urge it now, it will receive a very sad blow.

The American Metrological Society, after discussing the same proposition as made by our Boston friends, voted that the time had not come when any compulsory legislation was advisable. It was thought, however, that the action of the Government should be shaped in certain respects towards the use of the metric system. It is in use now in certain public works of our country, as has been remarked, and might be more generally introduced in such. It should be used to some extent in the Custom Houses in levying duties upon invoices expressed in metric units, where now a great amount of clerical work is done, to convert such invoices into American measures, while at the same time it would be entirely out of place to require the use of the metric system for invoices from Great Britain. Let it be employed in all cases where it is preferable to the usual system. Metric measures are legal; the law is already made; it only requires adoption in common use. The place the friends of the cause will find it useful to seek legislation, is at home, in their own States, where they have influence with the Legislatures, to the end that the metric system may be taught in the schools. There is where we must begin. It was never a habit of the Anglo-Saxons or of the Americans to make laws in order to create customs; we always have the customs first, then make the laws to conform—hence make this a custom.

Mr. Herschel.—I would ask the gentleman, whether he favors or opposes the resolutions?

Mr. Hilgard.—It would be difficult to dissent from either of the resolutions offered; the second one I could vote for readily. I should also be inclined to vote for the first, but for my official position as Warden of the Standards, which imposes upon me a certain reserve.

Mr. Theodore G. Ellis.—The subject of this resolution embraces a very large field for discussion, and I do not feel that justice could be done to it in the very brief and limited space of time that can possibly be given to it in this Convention. The paper by the committee of the Boston Society of Civil Engineers, presented to the Society for its action, recommending the adoption of the metrical standards, seemed to me to be a very fair presentation of the subject, and on the first glance the adoption of the French measures would appear to be a great benefit. That a change from our present complicated system of weights and measures is desirable cannot be doubted; but what that change shall be, opens a question that is not so easily disposed of.

The advantages possessed by the metrical system are: it is a purely decimal system, and it is, at the present time, the legal standard of more than twenty different countries. There are some other minor advantages claimed by its advocates, which I will leave them to set forth.

Its chief disadvantages are: it has not yet, with all the advantages claimed and all the force of legal enactments, come into general use in any one country; its unit of length is an inconvenient one. There are other inconveniences resulting from the length and subdivisions of the unit that will be named hereafter.

Is a purely decimal system the best one for ordinary use, including all the daily uses for weights and measures, for scientific and mechanical purposes as well as in trade? This may be squarely questioned at the outset; no nation or class of people has yet adopted it. In our own country we point to our coinage as a decimal system, but it is not one at all. Our system has practically but two units,—dollars and cents; the latter of which is one-hundredth of the former. Less than a cent is always spoken of as a fraction of a cent, and the dollar is also divided into halves and quarters in the transactions of common life. Although we have not had a coin representing the eighth of a dollar for twenty years, we still see "12½ cents a yard" in the shop windows, and Adams' Express Company to this day make charges in eighths of a dollar. In the United States coinage, halves and quarters have always been used, and I do not know of a 20 or a 30 cent piece ever having been coined.*

* Since the above was written, it has been learned that a few 20 cent pieces have been coined, but from their utter uselessness have never come into general circulation.

The only thing "decimal" about our money is the centennial relation of the dollar and cent, which allows the ordinary arithmetical operations to be performed as in abstract numbers, by keeping the proper position of the dividing point.

In France, where the metrical system has a new name for every ten units of the lower degree, it has been found impossible to force the system upon the common people. In the stores in Paris, at the present day, may be seen everything at so much the "half kilo," and the "half kilo" is divided into halves and quarters. The "half kilo" is almost exactly the old French pound, and still holds the affections of the people. In the dry goods shops, they are obliged to measure by the metre, but when the customer tells him how much is wanted, the obliging clerk makes the computation in common fractions.

I venture to say that no purely decimal system ever can be used in common life. A system of two units, something like our dollars and cents, may be, however, practicable.

We are, however, more interested in the question in a scientific point of view: whether it is better for us to continue the use of the old measures than to make a change which may be a somewhat better system, but not by any means the best that can be devised. The fact that in many countries all the scientific works will refer to these weights and measures, is certainly a great argument; but it must be likewise borne in mind, that the daily use of an inconvenient system would be a greater evil than the occasional conversion of quantities required in reading a foreign book. With the engineer all quantities are "decimal;" we measure by feet and decimals, our surfaces are measured by square feet and decimals, and our solids by cubic feet and decimals. What, then, do we gain by substituting the metre for our unit of length? Nothing but the additional trouble of conversion into feet, if we happen to want that measure. The trouble of converting into gallons, yards, inches, or any other measure, would also be greater, if it were required. The foot, therefore, as a unit, seems to offer to the engineer all the advantages of decimal notation that are supposed to be derived from the use of the metre. The only possible advantage that the metrical system has over the foot and its decimal divisions is where we want to know the weight of a certain cubic quantity of water, or rather distilled water, at 39° Fahr. temperature. For all other measures of length, surface, capacity or weight, the foot system appears to have all the advantages of the French system.

In this connection let us for a moment look into the scientific accuracy of the French system; to see if it possesses the perfection claimed for it by its admirers. The metre was originally intended to be the ten millionth part of the quadrant of a great circle of the earth on the meridian passing through Paris, but on account of erroneous calculations, the standards constructed proved to be far from that ideal; so that the length of the metre was declared by law to be a certain part (443.296 lines) of the "toise de Perou," and a platinum bar was declared to be—at zero centigrade—the standard metre. This bar is deposited in the "Conservatoire des Arts et Metiers" at Paris. The "litre," or unit of capacity, was originally intended to be the volume of a cubic decimetre, and the "gramme," or unit of weight, was intended to be the weight of a cubic centimetre of pure water at its temperature of maximum density—about 4° Cent., equal to 39.2° Fahr,—weighed in vacuo. Owing, however, to imperfections of workmanship, and the great difficulty of determining these quantities, the standards were finally fixed by law, making a certain piece of platinum the standard kilogramme, and the litre to be a kilogramme of water at the standard temperature of 4° Cent., and barometer at 760 Mm. This makes the kilogramme, one 120 000th less than its theoretical value and the litre, 0.999 992 cubic decimetre. So much for beautiful theories coming down to actual masses of metal like the British standards. Quite a number of standard metres were made, and some of them found their way to this country. No two of them were exactly of the same length, and being not very accurately made, and all measures "abut," with their ends not square with their length, we never knew how long our Coast Survey metre was, until our standard was carried over in 1867 and compared with the French bar.

One of the chief objections to the metrical system, at least for the use of engineers, appears to me, to be the length and size of the units. The metre is too long for a single measurement to be laid off by hand at once. No multiple of it is convenient for the length of a measuring pole or leveling rod, and no multiple is convenient for the length of a chord in laying out curves by angles. Our "foot rule," our "10 feet pole," and our "100 feet chain," are vastly more convenient lengths, and these are *decimal measures* which no *convenient numbers of metres* could be. 1 metre is too long for a rule and too short for a pole, 10 metres is too long for a pole and too short for a chain, and 100 metres is too long for anything. The litre is perhaps a convenient quantity, but the

gramme is too large for a scientific unit, as in assaying and chemical use, and too small for commercial purposes. The milligramme being the smallest recognized decimal, we almost invariably see such expressions as one-tenth, one-half, one-fourth, &c. of a milligramme.

The progression by tens, in the areas, capacities and weights, is also a great objection to what is now called the "metrical system." The areas should be squares of the units of length, and the units of capacity and weight should be based upon the cubes of the same measures. The units should, moreover, be of convenient size for common use, which is not the case with the metric system.

Another chief objection to the introduction of the metrical measures is, that the *English inch* has become the standard throughout the whole civilized world, even in France, for all the nicer mechanical gauges and measures. In the great machine-shops of this country, these standards cannot, and in my opinion never will, be supplanted. It is too late for any such innovation.

If we are to attempt to change our system of weights and measures, let us have the best system possible. Let the change be radical, and entirely eradicate all objections to the present inconveniences. Let us hear no more of degrees, minutes and seconds; of hours, minutes and seconds. Let us have a coinage of weight and diameter conforming to the standard adopted and that will be the same for all civilized nations. Although the French measures may be thought by some to be better than those which we now possess, it is hardly worth while to change until such a time as we can get the best possible system, and arrange to have a common standard with Great Britain, the most important foreign country to us who speak the same language and read the same books. The British foot and inch have such a hold upon the affections of all who speak our language, that it is extremely doubtful whether any other units can be forced into general use.

The metrical system was introduced into France by the law of 18 Germinal Year III, which is "metrical system," for April 7th, 1796. The metre was declared to be "Vrai et definitif," in 1799, and the system was established by law, November 2d, 1801. July 4th, 1837, the French government, finding the introduction of the metrical system a total failure, passed a very stringent law making the use of the new measures obligatory under heavy penalties, to take effect January 1st, 1840. Now we all know whether such an enforcement could be made in

the United States. If we can find a system that is superior to what we have, and the use of which will be more convenient than our present measures, it might be adopted, but it must commend itself by its merits or it will not be used.

The use of the metrical system was legalized in the United States on July 26th, 1866; now there is nothing to prevent the admirers of this system from using it to their heart's content—they can measure by it, compute by it, and contract by it, without let or hindrance, and with perfect legality, yet how many have done this? The law of the United States likewise ordered that the Post Office Department should regard 15 grammes as the half ounce standard for letter postage, and ordered that metrical balances should be distributed to the different offices, yet no notice was taken of this by the Post Office Department.

It is very difficult to see what the advocates of this system expect to gain by making the metre the only legal standard at some distant day. It is now three-quarters of a century since it became the legal standard of France, yet it is not thoroughly introduced there even at this day; if commenced here now, there is a long and dismal time of confusion to look forward to, and one which we shall never live to see completed.

It is difficult also to see how the making of a law by Congress, establishing the new standards, will help those who wish to use them. Congress cannot re-write all our books, tables, and formulas, which are now based upon the foot and inch; it cannot change all the standards and gauges used in our machine shops; 10, 20, 30 threads to an inch, for instance, would be no even parts of a metre; one-quarter, one-half, three-quarters inch, &c., would have no common measure in metres; in fact all our common sizes of screws, pipes, iron and steel, and all our gauges and standard sizes, would require to be altered. The immense labor and cost of this, and the time that it would take to accomplish the change, can only be appreciated by practical men engaged in mechanical pursuits.

To all intents and purposes we have at the present time in this country as complete a decimal system as the metric system would give us, that is, so far as we as engineers are concerned. The foot and its decimals are invariably used by civil engineers, and the inch and its decimals are invariably used in our machine shops; the pound and its decimals is our common weight. Any one who has had any experience with these units and with the French system, knows which are the best and most practical units. The incongruities of Troy weight, apothecaries' weight, avoir-

dupois weight ; miles, furlongs, acres, roods and rods, should undoubtedly be at once abolished by a stringent law making their use illegal, and our liquid measures should be made to conform to the foot measure; but these changes could be readily and easily made, and would be rapidly adopted.

That a reform should be made at the earliest practical time in our present weights and measures, is, I think, evident to all ; but in making the change we want the best system that we can have, to suit all the wants of the community ; this the metrical does not furnish. In my opinion we should assume for our base the British foot. I say British foot, because from the intermediate comparison through the metre by Mr. Hassler of the Coast Survey, the length of the American foot is somewhat problematical, and has never, I believe, been fixed by law.

In the establishment of the system of any weights and measures there is one point of sufficient importance not to be overlooked. This is the fact: that although a decimal system is the most convenient for computation and for all scientific purposes, it is not readily comprehended by the common people, who all have to wrestle with groceries and provisions ; they do now in all parts of the world, and always will, subdivide units by natural fractions. This, it appears to me should be taken into account in the establishment of any system of weights and measures. It is very unfortunate that the ancients counted their thumbs in establishing the base of the present arithmetical system ; if they had only left them out and counted their fingers, what a vast amount of trouble, vexation and work would have been saved to the world. In addition to the simplification of our arithmetic, we could have had a system of weights and measures capable of continuous sub-division, with the fractions all represented by one significant figure : thus the fractions : .5, .25, .125, .0625 .03125, .015625, &c., from $\frac{1}{2}$ to $\frac{1}{64}$ would be represented by : .4, .2, .1, .04. .02, .01, &c., the base would have been a perfect cube and its half a perfect square.

The decimal system has now become so universal that perhaps its use cannot be superseded, but the subject is worth consideration. I think that we should be in no haste to recommend the adoption of the metrical system, because we can have a better one without varying materially from our present standards, and there is no good reason why we should adopt the metrical system, except that it is now used by many other countries, an argument that materially loses its force when we consider that, with the exception of France and Germany, it makes little or no difference what standard of measures they use, and even these countries

are not allied to us like England by a common language which renders its printed works as well known as our own.

If an international commission, composed partly of engineers and practical as well as educated men, instead of being composed entirely of impractical scientific men, professors in colleges and geodesists, as has generally been the case, could be appointed to determine this matter, I should be heartily in its favor; but I am decidedly opposed to any action which shall inflict upon us all the inconveniences of the metric system unless it becomes fully and finally determined that we cannot have a better.

MR. ROBERT BRIGGS.—I desire only to say, without entering into the question of the merits of the metric system, or the propriety of voting upon them, or of admitting them and voting to commit the Society for or against the immediate or prospective adoption of the system, that it is better, now at this time, to go no further than to refer the subject to the Society for discussion. More light should be thrown upon it to allow some, perhaps many of us, to decide how to vote. It is unnecessary to call for a letter ballot with the present information of the members on the merits of the question, or with the present interest which they have in the result. If we are going to have a letter ballot, the members of the entire Society should be educated, instructed, interested generally, so that there would be a full count of the vote from those who understood distinctly the grounds upon which they will have voted.

This incomplete discussion, or any discussion before the few members here present, would be a very unsatisfactory showing of the views of many of our members, or presentation of the facts of the case however well reported, upon which to ask the ballot vote of the Society. It seems to me that the greatest good will be done by deferring the consideration. But few of those who are present have, I think, from the present discussion come to understand the question so as to vote upon it intelligently. It had better lay over.

MR. HERSCHEL.—I fancy there is some misapprehension about voting on this matter by letter ballot. It seems to me clearly to be the proper way. The discussions can be printed and put before members, and the report of the committee may then be made; but if deemed advisable, the letter ballot may be deferred for six months.

MR. BRIGGS.—The last resolution reads that the Chair appoint a committee of five from the Society to send a memorial to Congress in furtherance of the metric system. Now, if we desire to adopt this resolution,

with or without letter ballot, but without further discussion, the result will be that few of the members only will understand—or voting, they must vote without understanding—the subject at all.

We had better vote down the resolution, or defer it. The important question to be considered is, whether it is proper and best for this Society to lay before its members the reasons, the why and wherefore, for the introduction of the metric system. This discussion we have had here is wholly insufficient. I have myself a little manuscript of possibly 60 pages, which, if this were the time and place, I should like to offer as my contribution. John Quincy Adams said of the metric system, fifty years ago, after advocating the system in the warmest manner, that he could not see why it should not be adopted. There is another report of Prof. Charles Davis upon the opposite side, while Prof. Barnard takes up the question with the warmest advocacy. Now, I do not believe that the members of this Society generally, especially those who have not already come to appreciate the advantages of the system, know of even the existence of these reports, much less of their tenor. If we vote for this resolution for the forcible, or immediate, or compulsory introduction of the metric system, we shall have made the great mistake of risking defeat by asking a vote on the wrong grounds.

MR. HERSCHEL.—The resolution does not propose compulsory action. It does not say so at all.

MR. BRIGGS.—The inviting the action of Congress implies compulsion. But even taking the resolution as it stands, the question does not seem to be in a shape to be voted upon intelligibly by the Society at large. I would propose that the resolution be voted down, when I would move that the President appoint a committee of five to report upon the metric system; I make this proposition solely to arrange to leave the question open, and do not wish to be in any way discourteous to the mover of the resolution, however erroneous in construction I deemed it.

ERRATA.—On page 317, line 5 from the bottom, for "fenders" read "fundus."

AMERICAN SOCIETY OF CIVIL ENGINEERS.

INCORPORATED 1852.

TRANSACTIONS.

NOTE.—This Society is not responsible, as a body, for the facts and opinions advanced in any of its publications.

CXXIX.

A CHEAP TRANSFER TABLE.

A Paper by WILLIAM P. SHINN, C. E., Member of the Society.

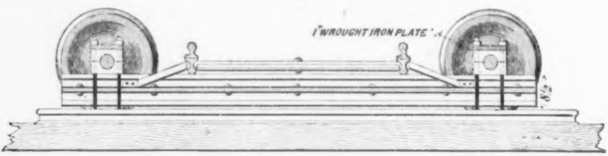
PRESENTED JUNE 5TH, 1876.

Having occasion a few months since to construct a transfer table, to be used in connection with the new machine shop of the Alleghany Valley Railroad Co., at Verona, I conceived the idea of making it of steel rails, using 2 rails, placed base to base and riveted together to form the main beam, and pieces of similar rails similarly arranged to form the cross-beams or trucks, resting on the axle boxes of the wheels, upon which the table moves. The plans in detail were prepared by Mr. John C. Lewis, Engineer-in-Charge, and the result has been so satisfactory that I have thought it a matter of sufficient general interest to present this brief description, accompanied by drawings, showing the details of construction of the table.

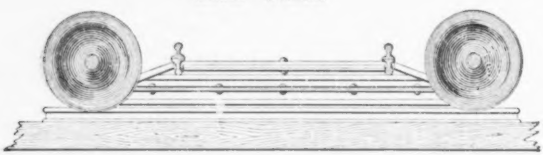
The main beams running longitudinally of the table, are each composed of 2 steel rails, 60 pounds per lineal yard, riveted base to base, forming a beam 8 inches high, as shown in the section and end view, (Fig. 1, next page), the top rail of the beam being used for the engines to run upon, thus saving the cross ties and the additional rails ordinarily placed upon the table, a saving not only desirable in point of expense, but also in point of weight. The cross-beams forming the trucks are

made of steel rails of the same weight and section, riveted together in the same manner, and suspended by hangers or stirrups of one inch round iron, from the axle boxes of the wheels upon which the table runs, as shown in the end view.

Fig. 1.



END VIEW.



SECTION THROUGH A. B.

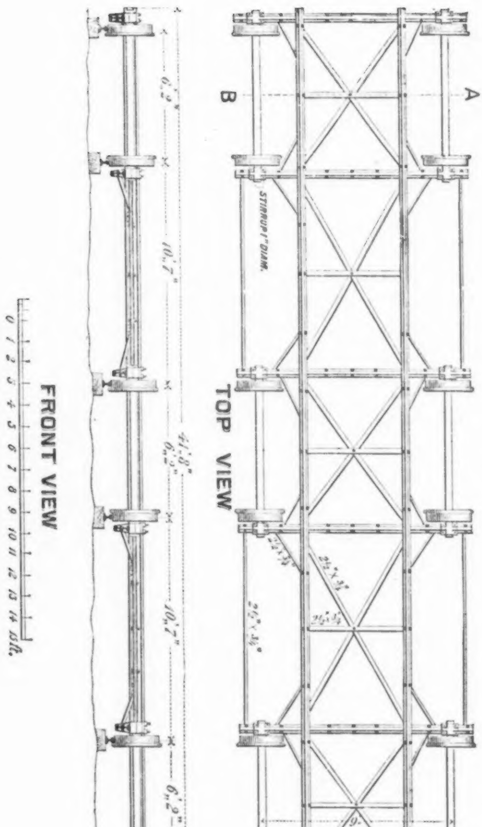
The spans between the truck-beams are braced transversely in the center, and diagonally by iron bars $2\frac{1}{2} \times \frac{3}{8}$ inches, riveted through at their point of intersection, while the truck-beams have a strut-brace outside of the longitudinal beams, on each side of each truck-beam, and a longitudinal brace between the ends of the truck-beams ; all these braces being of iron, $2\frac{1}{2} \times \frac{3}{4}$ inches, and riveted at their ends.

The spans of the trucks were made to conform to track walls which had been previously built, and which were 6 feet 2 inches from center to center, having the spans between the trucks 10 feet 7 inches from center to center. Had the track walls not been built, I should have had them spaced equally, making the spans each 8 feet from center to center, which would have afforded a better distribution of the points of support.

The truck wheels are 26 inches in diameter, and the axles are 3 inches in diameter, having $2\frac{3}{4}$ inch-journals, 8 inches long.

The weight of the table complete is as follows :

Axles	2 022	pounds.	
Wheels	4 980	"	
Bearings, brass	108	"	
Rails, steel	6 000	"	
Boxes	562	"	
Braces	1 000	"	
			14 672 pounds.



CHEAP TRANSFER TABLE.

Fig. 2.

The cost, as shown on the books of the company, was $633.03—as follows:

Axles, (second hand)	$36 00	
Wheels " "	114 00	
Bearings, 1st quality brass	48 00	
Steel rails, "	179 86	
Boxes, cast iron	15 17	
Labor, erecting, fitting, &c., complete	240 00	
		$633 03

The rails were furnished by the Edgar Thomson Steel Co., Limited, rolled and cut for the main beam to the exact length required, 41 feet 8 inches, and the fitting up was done by the Keystone Bridge Co.

The first engine run upon the table was a wrecked engine, weighing 34½ tons. The trucks having been knocked from under it in the wreck, the whole weight was concentrated on the two drivers, which rested on one of the 10 feet 7 inch spans. Under this weight, the deflection was ¾ inch on the span. When the weight was placed in the center of the truck span, the deflection was less than ¼ inch, and both spans returned to their original position upon removal of the weight. Had the truck beams been spaced equally, it is probable that the deflection would not have exceeded ¼ inch under the concentrated weight of the engine referred to.

The table has now been in use three months, and has given entire satisfaction.

Should there be any difficulty caused by undue deflection of the long spans, it can be remedied by introducing a truss rod, each side of each truss beam, at a cost not exceeding $40. 33 inch-wheels would have been better for the trucks, than the 26 inch-wheels used, the latter having been used because they best fitted the height of the walls. With the modifications referred to—that is, an equal spacing of the track walls, and the use of 33 inch-wheels—this plan of table would be found, I think, to be the most economical of any in use.

CXXX.

EFFICIENCY OF STEAM VACUUM PUMPS.

A Paper by J. Foster Flagg, C. E., Member of the Society.

Read December 1st, 1875.

The object of the present paper is mainly to point out a simple method of obtaining the efficiency of the class of steam pumps upon Savary's old plan, variously denominated, "steam vacuum pumps," "pulsometers," "aquometers," etc., and at the same time to show, notwithstanding the imperfection of the data given of the experiments upon one of this class, their great inferiority in economy of power to ordinary piston or plunge pumps, especially for great lifts.

The experiments were hastily improvised near the close of the Cincinnati Exposition of the present year, by the writer, in a spare interval between more important tests upon which he was engaged. It is a source of special regret, that no experiments were made with a low lift, say of 10 feet above the pump. After working up the data taken, an effort was made to pursue the investigation in this direction; but the Exposition had closed, and although the owners of the pump were desirous of having further tests made at their works in Cincinnati, yet as they could not provide simple but suitable arrangements as were necessary for the purpose, the idea had to be abandoned.

The motive power of the machinery hall, at the Cincinnati Exposition, obtained its steam mainly from two pairs of boilers, one pair being located at each end of the hall; they were connected by a 4-inch steam pipe well protected with asbestos cement.

The pump experimented upon was located almost exactly midway of the length of the steam pipe, thus drawing its supply of steam about equally from each pair of boilers. Its water was drawn directly from the canal, at the Exposition door, through a 3-inch pipe, 155 feet long, with 12 elbows in it, the lift being about 10.83 feet from surface of canal to center of pressure gauge.

The experiments made were each of half an hour's duration. An assistant was posted at each pair of boilers to take the steam pressure, making his observations at five minutes interval; a water gauge was attached immediately above the pump, and the cock in the discharge pipe partly closed, so as to produce the requisite pressure upon the pump corresponding to the desired lift; the pressure indicated by this gauge was

also noted every five minutes, and the temperature of the effluent water taken. The temperature of the water in the canal, which varied but slightly, was also taken. These constituted all the requisite data for the problem, excepting a rough estimate of discharge for calculating the friction in the long suction pipe, and the quality of the steam for obtaining which, a number of experiments were made upon each pair of boilers* in the course of testing some automatic engines a few days previously, and which for the purposes of this experiment was assumed to be the same as then determined.

The results obtained would doubtless have been more accurate, could the temperature of the supply water in suction pipe have been taken close to the pump, there being an element of uncertainty in taking it at such a distance as it was necessary to do in this case; if the temperature of the water were much increased in flowing this distance, the apparent efficiency of the pump would be materially less than the actual efficiency. The pipe, however, was laid under ground, there were no fires in the building for heating purposes, and the pump was run for some time before the experiments were commenced; so that although it is probable that an increased efficiency would have been shown, had the temperature been taken contiguous to the pump, the difference, in the writer's opinion, would not have been very great.

THEORY.—The following formulæ are but a modification of that for obtaining the quality of steam in a boiler, the novelty, if any, being in its application to the obtaining of the efficiency of a motive power.

The increase of temperature of the water in passing through the pump, as the steam used is entirely condensed in this water, is an accurate measure of the steam consumed per unit of water elevated; and the lift being known, the useful effect obtained from each unit of steam can easily be calculated.

Let N = total number of heat units imparted to 100 pounds of water by the steam used in elevating it;

x = pounds of dry steam consumed in doing the work;

u = ratio of water carried over from boiler with steam,—whence

$u\,x$ = total water thus carried over;

L = latent heat of the steam;

t = difference in temperature of steam and of water passing out of pump;

H = total lift of water, including friction in suction pipe, and

h = lift above pump.

* As described by Prof. R. H. Thurston. Transactions, Vol. III, page 297.

Then the actual units of heat consumed in doing the work

$$= \frac{100\,H + x\,(1+u)\,h}{772};$$

$$N = x\,(L+t) + u\,x\,t - \frac{100\,H + x\,(1+u)\,h}{772}; \qquad (1.)$$

$$x = \frac{N}{L + t\,(1+u)} + \frac{100\,H + x\,(1+u)\,h}{772\,[L + t\,(1+u)]};\ \text{or} \qquad (2.)$$

$$x = \frac{772\,N + 100\,H}{772\,L + (1+u)\,(772\,t + h)}: \qquad (3.)$$

but the last term of Eq. 2 is so small that it may ordinarily be omitted, so that

$$x = \frac{N}{L + t\,(1+u)}. \qquad (4.)$$

For pounds of steam required per horse power per hour, we have

$$Q = \frac{1\,980\,000}{100\,H + x\,(1+u)\,h}\,x. \qquad (5.)$$

Assuming an evaporation of 9 pounds of steam to 1 pound of water, we have for coal required per horse power per hour—

$$C = \frac{220\,000}{100\,H + x\,(1+u)\,h}; \qquad (6.)$$

and for duty of 100 pounds of coal

$$D = \frac{900\,[100\,H + x\,(1+u)\,h]}{x}. \qquad (7.)$$

If the pump is a moderately efficient one, $x\,(1+u)\,h$, may be omitted in the above formulæ, giving us more simply,

$$Q = \frac{19\,800}{H}\,x; \quad C = \frac{2\,200}{H}\,x; \quad D = \frac{90\,000\,H}{x}. \qquad (8,\,9,\,10.)$$

EXPERIMENTS.—The water gauge used was one of Ashcroft's, the thermometer, a standard one of James Green's.

FIRST EXPERIMENT. OCTOBER 8TH, 1875.

Time.	Temperature. Effluent Water.	Pressure. Water Gauge.	Boiler Pressure.	
			East.	West.
A. M. h. m. 11.45	86°	38	75	76
50	86	36	76	76
55	84.5	35	74	77
12.0	88	32	71	67
P. M. 12.05	87	31	66	64
10	88	35	67	68
15	89	40	75	74
Average....	86.93	35.29	72	71.71

Average boiler pressure, 71.85 pounds; temperature of water in canal, 60°; barometer, 29.5 inches; weight of water at 87° temperature, 62.122 pounds per cubic foot; and water gauge pressure, in feet of water, 81.81 feet.

SECOND EXPERIMENT, OCTOBER 8TH, 1875.

TIME.	TEMPERATURE. Effluent Water.	PRESSURE. Water Gauge.	BOILER PRESSURE.	
			East.	West.
P. M. h. m. 2.50	73°	15	57	51
55	73	15	59	51.5
3.00	73	15	57.5	50.5
05	73	15	57	50
10	73	17	61	53
15	73	18	64	57.5
20	73.5	19	65	60
Average....	73.07	16.29	60.07	53.36

Average boiler pressure, 56.71 pounds; temperature of water in canal, 61°.5; weight of water at 73° temperature, 62.25 pounds; and water gauge pressure in feet of water, 37.68 feet.

The calorimeter test gave for every 100 pounds of wet steam—for east boilers, 78.4, and for west boilers, 81.5 pounds of dry steam; the average being 80 pounds of dry steam, giving for u the value, 0.25.

The steam drawn to obtain the above was taken from the pipes near the engines, at some distance from the boilers.

From a careful comparison of steam gauges located close to the steam chests of neighboring engines, with the boiler gauges, an allowance of 6 pounds was made as the proper difference of the pressure of steam at the boilers and at the pump.*

RESULTS. FIRST EXPERIMENT.—The delivery of water was roughly measured in the second experiment, for the purpose of obtaining the friction in the long suction pipe, but it was overlooked in this experiment. The pump was working very slowly, owing to the load imposed upon it, and the velocity in suction pipe was estimated, judging from the measured velocity in the second experiment, at 1 foot per second; this would give a head due to friction of 0.36 feet.

We then have the following elements for substitution in the formulæ: $N = 100 \times 26.93 = 2\,693$; L (at pressure of 65.85) $= 894°$; $H = 81.81 + 10.83 + 0.36 = 93$; $t = 312.28 - 86.93 = 225.35°$; $h = 82.81$; $u = 0.25$: from Eq. 2. $x = 2.3$ pounds (the last term of this equation adds but 0.01 pounds to the value of x); and from Eq's 5, 6 and 7, the quality of steam per horse power per hour, $= 477.5$ pounds; the coal per horse power per hour, $= 53.06$ pounds; and the duty of 100 pounds of coal, $= 3\,732\,260$ feet pounds.

* An error of 10 pounds in steam pressure would only affect the calculation of the amount of steam used, about 0.5 per cent.

Results. Second experiment.—As above stated, the discharge was roughly measured, giving a velocity in suction of 1.51 feet per second, and a resulting head, due to friction of pipe and bends, of 0.75 feet.

We then have the following data: $N = 100 \times 11.57 = 11.57$; L (at pressure of 50.71) $= 904.2^\circ$; $H = 37.68 + 10.83 + 0.75 = 49.26$; $t = 298.17 - 73.07 = 225.1^\circ$; $h = 38.68$, $u = 0.25$: from Eq. 2; $x = 0.981$ pounds (the second term adding 0.005 pounds); and from Eq's 5, 6 and 7, the quantity of steam per horse power per hour, $= 390.7$ pounds; the coal per horse power per hour, $= 43.41$ pounds; and the duty of 100 pounds of coal, $= 4\,561\,200$ feet pounds.

The radiation of heat from the pump chambers is comparatively small, and has been neglected; it is partly offset, at least, by a similar radiation from the small unprotected steam pipe connecting the pump with the main pipe.

The heavy loads imposed upon the pump in both experiments caused it to work so slowly that much more steam was undoubtedly condensed while the water was being forced from the chambers, than would be the case with a low lift and rapid movement of the pump. Moreover, the greater the vertical suction given to the pump, provided it be not so great as to impair the prompt movement of the water into the chambers, the greater will be the efficiency shown, as this lift is obtained entirely by the condensation of the steam.

An experiment therefore, made with the maximum profitable height of suction, with a low lift above the pump, not exceeding say 10 feet, and with the water drawn from a reservoir directly underneath, so that its temperature entering the pump can be known with certainty, would undoubtedly give a much greater efficiency; and using a thermometer with a small range, but with large divisions on the scale, so that small fractions of a degree can be estimated with accuracy, this method will give a close approximation to the true economic value of one of these pumps.

These experiments, imperfect as they are, will show at least the probably very low duty of this pump for any height of lift, and a rapid decrease therein for high pressures.

Since writing the above,* the author, through the kindness of Charles Latimer, Esq., the Chief Engineer of the Atlantic & Great Western R. R., has had the opportunity of making farther experiments upon a pump of this class, of a different and well-known make, used for supplying the tank at a water station on the line of that railroad.

* November 1st, 1875. The following was written December 31st, 1875.

Most of the circumstances were favorable for obtaining accurate results, though perhaps not so for the greatest economy of which the pump is capable; the condition, however, of ordinary service, in this class of work, would not indicate a higher duty than is here obtained.

The boiler and pump were located near the well, in a brick house built expressly for their accommodation. The suction pipe was only 24 feet long, and but 15 inches of its length was exposed (close to the floor) in the pump room. The suction lift (including friction) in the first experiment was 7.97 feet, and in the others nearly the same. The temperature of the external air was not very different from that of the water, although the pipe being well under ground it would not be affected by considerable difference therein, and it is certain that the temperature of the water as it entered the pump was practically the same as in the well.

The delivery pipe was 211 feet long, rising to the top of the tank, so that the height pumped was constant. The steam pipe leading from the boiler to pump was an inch in diameter, and about 13 feet long, and the pipe to calorimeter nearly the same length; so that the quality of steam, as delivered to pump and calorimeter, must have been nearly the same.

A cock was put in immediately above the pump, so that a small quantity of water could be quickly drawn, and its temperature noted.

Observations were taken once in five minutes for half an hour, the pump having previously obtained its regimen, and a calorimeter test was made at the beginning and end of the run. The results were as follows:

FIRST EXPERIMENT, DECEMBER 8TH, 1875.

Total lift from well to tank (including friction)	44.58 feet.
" " " pump " " "	36.61 "
Average temperature of water in well	40.6°
" " " above pump	50.93°
" steam pressure in boiler (quite uniform)	60 pounds.
" strokes (single) of pump per minute	51.8
" quality of steam (calorimeter)	68 per cent,
Coal per horse power per hour	40.2 pounds.

The pump was run as rapidly as possible and have it work steadily, the throttle having to be partly closed, owing to the pressure of steam.

The writer, thinking it possible that more economy might be obtained by working steam at a lower pressure with an open throttle, the experiments were repeated at a subsequent date, two tests being made, also of half an hour's duration each, the intention being to have the pressure at about 20 pounds during the first, and at about 41 pounds during the second experiment, at which latter pressure the pump would have had about its maximum speed as on the previous day. The throttle valve was constantly kept wide open during both tests.

SECOND AND THIRD EXPERIMENT, DECEMBER 23D, 1875.

	Second.	Third.
Total lift from well to tank (including friction).....	40.28 feet.	44.16 feet.
" " " pump " " "	32.83 "	36.18 "
Average temperature of water in well..............	47.5°	47.43°
" " " " above pump.........	56.14°	57.23°
" " " external air.................	51.5°	50°.
" steam pressure in boiler..................	20.8 pounds.	38.6 pounds.
" strokes (single) of pump per minute.......	33.4	50.14
Quality of steam (calorimeter)......................	81 per cent.	88 per cent.
Coal per horse power per hour.....................	39.78 pounds.	41.83 pounds.

REMARKS.—The boiler was many times larger than needed for the work, and the fire consequently had to be kept very low; moreover, the boiler was not supplied by an ordinary feed pump, but from a tank above, from which the water was periodically drawn by the fireman, steam being admitted from the boiler to the tank to equalize the pressure and allow the water to run in by gravity. The contrivance was crude, and a large amount of water was let in at once, at infrequent intervals.

The boiler was fed in this way during the first and second experiments, but not during the third, the result giving for the first two, a much lower calorimeter per cent. at the end of the run than at the beginning, and a considerably lower average than for the third. This, moreover, throws an uncertainty on the correctness of the calorimeter results. So although the results as a whole are probably very near the truth,—since a variation of 16 per cent. in quality of steam only varies the result about $1\frac{1}{2}$ pounds of coal to the horse power,—yet they are not sufficiently correct to judge at which pressure the pump is run most economically, farther than that probably there is but little difference at the different pressures tried.

As before stated, a higher suction and lower lift might give a little better results; but with lifts similar to those tried in these experiments, a consumption much less than 40 pounds of coal to the horse power can hardly be expected.

CXXXI.

PRINCIPLES OF TIDAL HARBOR IMPROVEMENT

AS APPLIED AT WILMINGTON, CAL.

A Paper by CLINTON B. SEARS, Corps Engineers U. S. A.
Member of the Society.
READ SEPTEMBER 6TH AND 20TH, 1876.

An eminent civil engineer, David Stevenson, referring to river and harbor works, remarks: "We must express our regret that although we have many treatises expounding the principles of engineering, nevertheless the engineers of the present day have given comparatively few accounts of the effects that have followed the application of these principles in particular cases."

The object of this paper is to bring together in concise shape, those leading principles of tidal harbor improvement and those only, which have received the sanction of acknowledged authorities; to show how they have been applied in a particular case, and the resultant effects. These principles are accepted without discussion, personal experiment and observation having satisfied me that they are essentially correct.

By a tidal harbor is meant a bay or estuary whose shore lines are sufficiently restricted to convert the ocean tidal wave into one of translation, thus producing what are known as tidal currents, and the flow and ebb of which alone can be depended on by proper treatment, to keep open the channel within, and the bar (if there be one) at the mouth. This eliminates from the problem all action of fresh water streams, and thus gets rid of an element very effective for good or for evil; the former obtaining by the utilization of a strong and steady current always flowing in one direction; the latter predominating when such current brings with it masses of sediment, whose proper disposition becomes a matter of great care and expense.

The most perfect type of a tidal harbor is one in which the waterway sections decrease gradually from the sea inland, and then expand into a basin of large comparative area, thus forming a reservoir for the temporary storage of the high tides. An example of this we have in the tidal estuary at Wilmington, California; (Plate I); the small map shows the high water area and the country in the vicinity, while the large chart

indicates the low water characteristics of the seaward portion of this estuary.

In any large operation for improving a harbor, too much stress cannot be laid upon the importance of making an exhaustive and accurate examination of the harbor and its conditions, as a preliminary step; in some cases as much as two per cent. of the ultimate cost of the improvement can be expended to advantage in such examination, and not only money but time should be amply taken for its accomplishment. "The designing of harbors constitutes confessedly one of the most difficult branches of civil engineering," and therefore the engineer must avail himself of his own past experience and that of others to enable him "to institute a comparison between the given locality and some other locality" whose conditions are well known and whose treatment has been successful. Such a comparison requires a careful topographical survey of the new work, for area, shape, &c.; a thorough hydrographic survey, with numerous and carefully plotted soundings reduced to the datum plane,—in the United States, that of *M. L. W.* (mean low water)—and which will give a reliable configuration of the bottom, and a consequent close calculation of sectional areas; borings sufficient in number and penetration to show the constitution of the sub-strata; an examination of the general geological conditions sufficient in extent to determine the manner of the original formation of the harbor; and last, but as important as any, a clear determination of the local action of the tide; its maximum and minimum range; high and low water areas of spring and neap; velocities, surface and sub for different stages and places; volumes of tidal prisms; amount of tidal incharge and discharge; direction of flood and ebb currents, both in and outside the harbor; and a theoretical inquiry into the probable effects of the intervention of any artificial obstacles in the paths of these currents.

For extended works in a large harbor, a year's time may be consumed profitably in a study of the tides, the other surveys being carried on at the same time. The general tidal characteristics of the open ocean are the results of well-known natural laws, and can be predicted with certainty; but when modified by depth, restricted shore limits and contracted area, it becomes necessary to give the tides special local study, and this should extend through a series of lunations.*

* To the engineer, desirous of studying in detail the subject of tides and tidal phenomena, I commend the exhaustive work of Prof. Ferrell, in the Coast Survey Report of 1874; and for a general study, several pamphlets by Prof. Hilgard, Asst. Coast Survey, and one entitled Reclamation of Tidal Lands by Prof. Mitchell, Asst. Coast Survey. I have found these works of great value to me in practice.

Generally three tide gauges should be used; one near the open sea, but protected from wave action, one near the middle, and one at the upper end of the harbor. If the estuary be long, narrow and tortuous, a greater number of gauges will be necessary, and all should have their zeros referred instrumentally to one level. For observations of any great extent, self-registering tide gauges will be found to be more economical and reliable.

When the improvement includes a construction exposed to the action of the waves, the direction of its axis should be as nearly as possible parallel to their direction, while at the same time meeting the other and paramount conditions required of it. Even a slight obliquity will greatly reduce their percussive effect.

If there be several entrances, one to remain open and the others to be closed, the choice should be the one that conforms most nearly with the direction of the prevailing fair weather winds, that sailing vessels may always make the port and go as far as possible inside without the aid of tugs, and at the same time be exposed as little as possible to the direct action of winter storms, which tend to move masses of sand across the entrance and form a bar—two requisites that are sometimes hard to reconcile. The entrance should be of tunnel or trumpet mouth shape, having the widest high-water section seaward, and gradually narrowing as it recedes inland, but the low-water sectional areas should contract as little as possible, and any artificial work to close an entrance and shut off the sea or sand, while conforming in plan to this requirement, should also follow the general contour of the work or shore on the opposite side.

The work in moving material, *i. e.*, the scouring power of water in motion, is in ratio to the square of the velocity, therefore for the moving force we must depend principally upon the spring rather than the neap tides, the rise and fall or range of the latter and the resulting velocities being comparatively small. Spring tides rise to the highest and fall to the lowest points; the tidal prism has its greatest range, and the current velocities are relatively very high. Of the flood and ebb tides, the latter is the one which is by far the more effective in scouring, its *vis viva* being from six to eight times that of the former; the spring flood starts only from a high low neap and runs to extreme high, while the ebb starting from this extreme high level, falls to extreme low water, *i. e.*, the flood rises, say from + 2 to + 7, a range of 5 feet, while the ebb falls from + 7 to – 1.5, a range of 8.5 feet, and this in approximately equal intervals of time.

The flood tide attains its maximum velocity about the end of the third quarter, and maintains it well into the fourth, its tendency being dispersive from the channel shoreward, spreading over the higher flats and seeking all the high level coves, indentations and minor estuaries; and "the farther up the tide may be enticed, the more powerful will be the downward flow of the ebb." The maximum ebb tide velocity obtains at the beginning of the third quarter, and the current retains a high velocity till near the end of the fourth, when it falls off quickly. The whole tendency of the ebb tide is concentrative and towards the last, every rivulet, creek and estuary in the tidal basin is being rapidly drained to fill the channel near the entrance, which in its turn is seeking the rapidly receding ocean level outside.

These relative characteristics of the flood and ebb tides indicate another condition to be observed in planning works for the entrance of a tidal harbor. They should be so arranged in plan and elevation, that they will not pond back or retard the last of the flood tide, thus preventing the full accumulation of water in the upper and higher reaches of the tidal basin, but allow free inflow, while at the same time they will direct the last of the ebb tide along one set channel, and thus take advantage of the best of its working force. A work whose top level is bare at half tide, will as a rule have the best elevation to accomplish this, and in plan the trumpet shaped or converging entrance aids the flood tide in its filling action.

In any system of training walls or low jetties for producing a scour in the channel or on the bar, their height should be in ratio to their distance from the entrance, those near the latter being the lowest, those most interior the highest; this insures the flood tide ready flow, affording a full channel and full upper reservoir, while the ebb is guided and concentrated for doing its best work in its last stages. In some cases it will be necessary to modify this rule by the precautionary one, that great single lengths of wall should go with low altitudes, and the converse; as in the upper reaches of an estuary, the channel may be narrower than in the lower, in which case the average section must not be lessened. In such instances walls should be used only at the wide places, so as to preserve uniform average sections. A system of training walls wisely planned can be made very effective in concentrating the currents to produce scour in shoal places, but if badly arranged they will generally make worse the channel they were intended to make better. "All the conditions of the hydraulic system of a harbor must be carefully investi-

gated, before undertaking to make any change in its natural conditions." The general object to be attained is to concentrate the tidal currents on one given axial line, without excluding tidal water from the estuary.

As care must be taken not to lessen the tidal influx by undue contraction near the entrance, equally important is it to preserve the integrity of the tidal reservoir within. "An estuary having a bar at its mouth or shoals along its channel, will receive injury if its tidal influx be reduced by encroachments upon its basin," unless corresponding increment is made in its low water section, to compensate for the reduction near the high water line. "The low water sectional areas increase directly as the volume of tide water that lies landward of each section line. The importance of preserving intact the capacities of the upper portions of tidal basins should be a matter of no doubt; for though it is true that contraction may benefit the navigation at the place where it exists, the effect cannot but be detrimental to the lower parts of the estuary" where shoals exist, and to the bar at the entrance.

In building a training wall, it should be put down as nearly as possible simultaneously throughout its length, thus avoiding the continuous pot hole liable to scour out at the free end and which will maintain itself to the final termination, thus greatly increasing the amount of material for construction, and endangering the channel below by shoaling it with the matter scoured out; in other words, a given volume of stone brush, &c., in a jetty, will serve its purpose better, by distribution in thin horizontal layers throughout its length, no layer being thicker than can be laid in one "heat," *i. e.* during slack water, than if deposited in vertical or inclined layers, the former method making a more gradual change in the hydraulic conditions of the channel at this point. Whether a series of training walls should be built simultaneously, must be determined somewhat by local circumstances, but as a general rule they should be built one at a time, beginning inside; this gives an opportunity to study their successive effects, and guards against the bad results liable to ensue from the sudden and violent change in the regimen of the channel incident to a simultaneous construction.

A main work designed wholly or partially to close one or more channels or water-ways, and where it lies across the direction of natural currents, ought not to be begun till ample provision has been made to stop or greatly mitigate the effects of the scour at the head of the work, in case it be founded in mud or sand. There should be provided for this purpose an abundant supply of broken or cobble stones, sand bags, brush,

&c., together with suitable rafts or light draught lighters for transporting and properly depositing them against the work at its head. It will often be necessary to prepare the line for some distance in advance of the main work, by sinking a strong wide mattress of brush ; this distance varying from 100 to 500 feet, depending upon the consistency of the bottom and the velocity of the currents. This precaution, though expensive, will prove a great economy in the end. If the work be of close piling, a strip along the axis of the brush-mat, equal to the width of the pile work, should be kept clear of stone ballast, that the piles may be driven closely and accurately. If, however, the pile-work be open and not accurately spaced, this care need not be taken, as the piles can be readily driven through a moderate thickness of ordinary sized ballast. I have known it to be done where the layer of stone was over 12 feet thick, and that without shoeing or even pointing the piles. In close pile work, however, a single cobble stone may cause a pile to creep off or split at the bottom.

Having constructed a system of training walls to guide and concentrate the currents, it may be necessary to accelerate the scouring by mechanical means, such as raking, dredging or blasting.

As submarine blasting is the most expensive of operations it should only be resorted to where absolutely necessary ; the points of application should be selected with great care and the exercise of sound judgment, so that the new hydraulic conditions due to the system of training walls may not throw the channel anywhere but through the cut blasted out.

Raking with a heavy iron rake or plough towed by or attached to a steamer, can often be used to advantage where a slight increase of depth only is sought, as where in a series of shoals each is followed by a pool of deep water ; or on a broad flat bar, a furrow may be started along the axis of a proposed channel, such furrow to be widened and deepened by the natural action of the currents. All raking should be done during spring tides, when the velocities are greatest, the rake being kept in the thread of the current and back and forth over the same line, as a narrow deep furrow will result better than a broad shallow one.

In laying out a line upon which to dredge, its selection must be such as to insure its coincidence with the resultant line of the working currents ; bends should be cut off or reduced in degree only when there is evident necessity.

The depth to which the dredging should be carried will depend mainly upon the extent of the improvement contemplated, and this should be governed by the effective working force of the currents, and

no artificial depth ought to exceed what this force may be relied on to maintain. This depth can generally be determined by the low water sectional area of some portion of the natural channel through which the whole force of the tide has been exerted, the conditions being the same. If only a stated sum can be devoted to dredging, it will be better to expend this in opening a deep narrow channel, than one wider and shoaler, trusting to the currents working in this channel, to widen it. The velocity of tidal currents increases directly as the depth, and depth is the great objective, as upon this essentially depends the amount of commerce, high authorities now agreeing that "the capacities for tonnage of different channels vary as the cubes of their depths."

The place of deposit for dredged material requires careful attention. Only under exceptional circumstances, should it be deposited inside the harbor, nor on the outside so near the entrance as to be in the range of possibility of being driven into the entrance even by the severest storm.

When owing to the great cost of long towage it becomes necessary to deposit inside the harbor, the reëntrant places along the shore line will be the best, and preferably those as far as possible from the channel. The material should be distributed so as not to reduce below the average the low water sectional area at any point; by depositing abreast of the point where excavated this will generally obtain, and can often be done, as the shoal places occur as a rule where the widest water-way exists, and it is at such points that dredging is most often necessary.

Where there are jetties, the deposit can be made with safety in the dead angles made by these with the shore line, care being taken to keep the top of the dump piles below the top level of the jetties.

Having now considered some of the most important principles to be observed in the improvement of a tidal estuary, where the currents are utilized to effect scour, we will proceed to a consideration of their application at Wilmington, California.

The physical characteristics of this estuary are as follows: (See small map, Plate I). At no distant geological period the ocean extended far inland beyond the village of Wilmington along the valley of the San Gabriel, now a very insignificant stream, but formerly quite a large river. The gradual emergence of the land, together with accretions washed down from the mountain ranges, has pushed back the ocean to its present limits. The bay shown on the map, is a series of flats exposed at low water and consisting of mud overlying clay and in spots, hard pan and beds of shells. To the east of the bay we find a wide stretch of sandy soil elevated but a

few feet above high water, and terminated by a range of bluffs, which latter end abruptly at the ocean, and have in the past extended much farther seaward than at present. Denudation and erosion by the sea has been going on for a long period and still continues. The composition is of very soft friable sandstone overlaid with adobe clay. This stone is readily and finely divided by the waves and surf, and soon becomes a mass of sand subject to their action and that of a littoral current which here sets up or westwardly, along the coast. Following along this shore to the west towards San Pedro, is a stretch of hard sand beach some 200 feet wide at low water and about 5 miles long; above high water the sand is piled into dunes by wind action, and has an average width of 500 feet. The first stretch of beach west of the bluffs, ends at a small estuary some 200 feet wide at high and about 30 feet at low water, where the depth is but a few inches. This inlet makes an island (called Rattlesnake) of the rest of the beach to the west, and is kept open by the water of the San Gabriel, which at low tide discharges through it. It shifts about within certain limits and varies slightly in depth, but is kept generally shoal by the waves breaking directly into its mouth, driving back the sand carried out and throwing in some of that drifting west from the bluffs. Only a small portion of this drift stops here, the mass being carried on to form and maintain Rattlesnake island, some 2 miles long, which shuts off from the ocean a body of water now known as the Wilmington estuary, at high water having a very respectable area.

The sand thus transported by the surf and littoral current has been thrown above high water level by the waves on high spring tides; the wind has then taken hold of it and swirled it into dunes, and has also brought with it the seeds of a peculiar creeping sand plant. This sprouts very soon and creates a small wind eddy round it, which aids in forming the dunes, and both grow together, the plant continually putting out fresh shoots; and the result is, that the island is covered with dunes, some of them 15 feet above ocean high level, and these are clad with a thick mass of green plants, which give stability to the surface.

The growth of the island westward was still going on in 1871, though very slowly, and its free end is known to have advanced towards Deadman's island several hundred feet in the last 50 years; and this would probably have continued till the creating force—the waves and surf—and the eroding force—the tidal currents round the west end—should be in *equilibrio*, a point they had almost reached at the time the United States began operations in 1871.

The hydraulic conditions of the estuary are these: inside is a broad shallow basin with a high tidal area of over 1 300 acres, which at *M. L. W.* is reduced to a series of tortuous shallow channels, the largest of which near Wilmington wharf has 5 feet at *M. L. W.* in mid-channel, and a top width of less than 150 feet. This channel going out keeps this average low water width for the next 3 000 feet, making several turns, and gradually deepens, till some 4 000 feet, from the wharf at *D*, (Plate I,) the 10 feet curve begins and continues out to the bar, the maximum depth being 20 feet. At *C*, the last of the lateral branches joins the main channel, making at *C G* a tidal gateway through which flows and ebbs the whole volume of the tidal prism. This concentration of tidal energy gives on this line *C G* a low water section some 800 feet wide, with a maximum depth of nearly 20 feet. At *X*, the island, which so far has acted as a jetty to guide the tidal currents, terminates, and ebb tide begins to escape seaward over the broad flat *X Y*, 3 700 feet wide and awash at low water; the greater volume, however, from the impetus received in passing *C G*, keeps the main channel, hugging that bluff, till it meets a hard pan formation at *F*, where it is deflected and makes its way to the ocean over the flat *Y Z*, 3 000 feet wide, and not more than 4 feet deep at *M. L. W.*, and through *A Z*, 2 000 feet wide, with a maximum depth of 7 feet. From about *C*, to beyond *F*, is the widest and deepest low water channel. After passing *Y*, the currents spread out and quickly lose their velocity. This broad flat shoal area *A F Y Z*—having not more than 1.5 feet of water over it at *M. L. W.*, even this depth being very irregularly distributed and presenting no well defined channel—constitutes the bar.

From *X*, to *Y*, the waves have for years been rolling in the sand, transported along the shore from the bluffs to the east; this sand has been carried across into the main channel, where the strong ebb tides have seized it and carried it out and dropped it over the area *A Y Z*, thus forming a drift-bar. The surplus sand not lodging here has been taken beyond the line *X Y Z*, to be returned again over the same path, on the first occurrence of a high spring tide coincident with a storm or heavy swell. This action being continuous, has essentially maintained the physical status of the mouth of the estuary as shown on Plate I. It is possible that after many years, if left to natural action, Rattlesnake island would have extended to Deadman's island; the channel within gradually cutting through the bar or pushing it bodily outside the line *A Z*.

This tendency pointed out the method of artificial improvement to be adopted, the object being to secure across the bar as deep a channel as

the tidal currents would keep open ; this method clearly appeared to be to hasten the natural action going on by closing the water-way *X Y Z*, and obliging the volume of water hitherto thus escaping seaward, to pass out with greatly increased velocity directly across the bar and through *A Z*. This would also fence off the sand along *X Y*, and prevent further deposit on the bar, and the artificial work along the line *X Y Z*, would become the nucleus against and around which the sand could accumulate and form a durable permanent barrier.

The line *A Z*, is the opening best adapted for an entrance to the harbor ; the hardest storms come from the south east, and the force of the waves as they approach the mouth is broken by Deadman's island, which makes an admirable abutment pier for the work, and as they roll in directly from deep water, they bring little or no sand from the ocean bed to block or shoal the entrance. The prevailing fair weather breezes coming from the west and south-west, sailing vessels can easily make the port and sail well inside without the aid of tugs.

Inside the section, *C G*, (Plate I), the channel gives off numerous meandering branches of very small low water sectional area, but the high water area, as said before, covers over 1 300 acres. The average spring tide rises 6.5 feet above *M. L. W.*, and falls to 15 feet below, giving an average range of 8 feet, and ranges of 9 feet in the tidal prism are not unknown. The general level of the flats is about + 3 feet, so that the average *clear* regular tidal prism on high springs is about 3.5 feet, making a volume of water over 198 000 000 cubic feet, or nearly 1 500 000 000 gallons, all of which passes out through the tidal gateway *C G*, daily on every course of spring tides, to say nothing of the volume of the irregular prism of tide water included between the levels + 3 feet and − 1.5 feet, a range of 4.5 feet. Through the tidal gap *C G*, the average maximum surface velocity is 4.4 feet a second, or 3 miles an hour, the sub-velocity somewhat greater and increasing near the bottom, the low-water sectional area being about 8 000 square feet. This velocity is maintained till well beyond the point or bend at *F*, when it falls off rapidly to 2 and over the bar to not more than 1.5 miles an hour, or 2.2 feet per second.*

By closing up the line *X Y Z*, we might reasonably expect to bring the velocity across the bar up nearly to 3 miles an hour, and with a bottom of like material as at *C G*, in time, scour out a channel with as great a depth as at the latter. This expectation, however, would have to be modified by the necessity of a converging entrance with much wider mouth

* I use the present tense as referring to the original status in 1871, and before.

than at *C G*, in order to meet two essential principles of tidal harbor improvement, and ensure the complete filling of the basin within, on every flood tide. Again, the condition of like consistency of bottom did not obtain, for extending from Deadman's island to the main shore at *A*, was a submerged reef of detached boulders, clay and hard pan, having over it at its lowest point 6.5 feet of water and 2.5 feet of sand; 9 feet depth at *M. L. W.* therefore, was all that could be expected across this, without mechanical cutting.

The axial line first adopted for closing the water-way *X Y Z*, is shown on Plate I, by the full double curve; this shape in plan conformed to the general outline of the opposite shore and gently guided the currents, but the axis as actually used is shown by the broken line, the work being thrown further seaward than was first intended, as it was feared the channel abreast of *Y* would be too much contracted. This was a mistake, as the line first adopted should have been adhered to; it would have saved much subsequent expense in training walls. The radius of the first curve was 2 000 feet, that of the reverse curve, 4 000 feet.

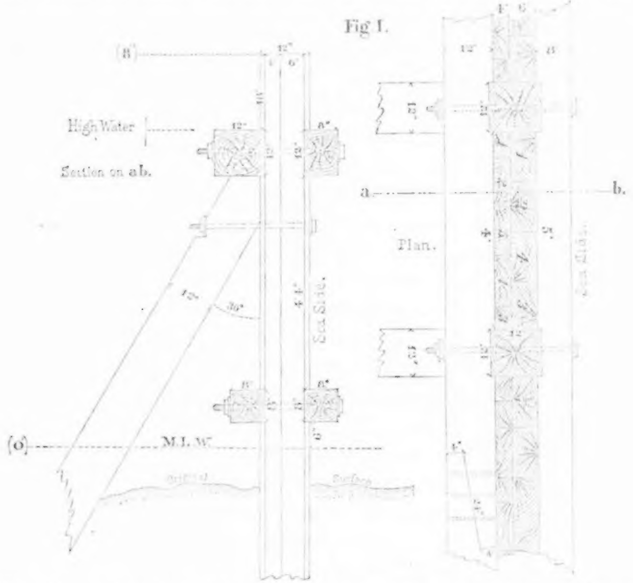
It was determined to build at least 4 700 feet of the work, of close piling, any other method of construction being too expensive. The level of 8 feet above *M. L. W.* was adopted as that of the top of the work. This, while shutting off the highest tides and breaking the force of the heaviest storms, would at the same time permit the ocean swell at high water to throw large masses of water over the work, which would have to find its way out over the bar, adding thereby to the scouring force; this swell would also heave over a good deal of sand, which, lodging against the work on the inside, would increase its stability and keep out the marine worms and the sand banked against the outside would fill there the same functions and also, in time, serve as a permanent breakwater after the decay of the timber.

This pile work consisted of three kinds of construction. The first, 100 feet long, having but one thickness of sheet piles, while the next, 3 600 feet long, had two thicknesses, breaking joints; (this latter is shown in plan and section by Fig. 1, next page.)

The first construction was wrong, and subsequently gave great trouble, and I caution others against it. Its fault was want of tightness; though carefully built, many cracks or openings between the piles necessarily existed; these allowed the water to work back and forth with the change of tide, and in consequence, at every crack there was a large pot-hole scoured out at each side of the work—often to great depths, sufficient to

threaten undermining. This was remedied at considerable trouble and expense, and was considered secure for nearly two years after construction, as the waves had banked the sand against it to the top and to the width of several hundred feet. Then came a storm, coincident with a very high tide, which sent the surf clear over this bank to the work,

Fig I.



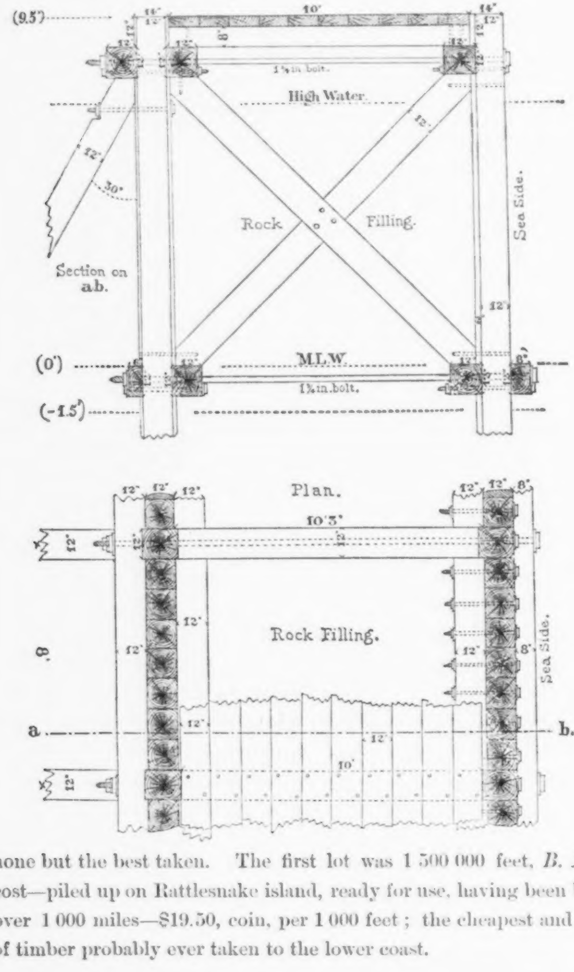
where it ran along back till it came to this portion; here the water found its way through the cracks that had been left unrepaired, for several feet from the top, washed out the sand, and finally worked round the initial point of the work, leaving a high-water channel some 3 feet below the top level of the work, and from 4 to 6 feet wide. To remedy this, a solid bulkhead was built at the end and on the prolongation of the axis, well into the island and as high as the surface of the latter, and the 100 feet of this faulty construction was filled up to the top on both sides with a solid mass of gravel and cobble-stones, and sand was packed behind.

At the end of the second construction, 3 700 feet out, where there was deeper water and the work was more exposed to the unbroken impulse of the waves, the construction (shown in section and plan by Fig. 2, next page) was adopted; this double work to be filled in solidly with

boulders and broken stone before the deck planks went on. It had a top level 1.5 feet higher than the single work, and the piles were 28 feet instead of 24 feet long.

The timber was Oregon fir, sawed at the mills to specified dimensions; each piece was carefully inspected by a faithful and reliable expert, and

Fig. 2.



none but the best taken. The first lot was 1 500 000 feet, *B. M.*, and cost—piled up on Rattlesnake island, ready for use, having been brought over 1 000 miles—$19.50, coin, per 1 000 feet; the cheapest and best lot of timber probably ever taken to the lower coast.

In the summer of 1871, a contract was made for the construction of the work—the timber, bolts, &c., being supplied by the United States—at the rate per linear foot of $3.15, coin, for the first, and $5.83, coin, for the second construction. No contract was made for the third construction. The contractor began work by driving the first pile, October 2d, 1871, and carried the work on as energetically as his knowledge and means permitted till he had completed some 347 feet, when, December 6th, he threw up the contract, declaring the work could not be built for any money. He had taken the contract too low, and tried to scant the work, but being obliged to drive the piles carefully and to the utmost possible penetration, he had to back out.

As the line advanced out from the shore across the track of the currents, the trouble from scour and undermining began, as much as 16 feet vertically of sand going out in one place on a single ebb spring tide, and leaving the work in a very precarious condition—holding on to the bottom in some places by only 4 feet of pile penetration. Energetic means were immediately taken to stop the scour and strengthen this portion of completed work, to enable it to stand the winter storms, most severe in January and February. At intervals of some 60 feet, on both sides, box cribs 24×6×4 feet were sunk at right angles to the work, being filled partly with loose sand, and on top with sand bags, made of second-hand grain and gunny sacks. Bundles of brush, loaded with sand bags, were put down alongside and against the work, and in one or two places, box cribs were sunk fore and aft; while at the head or free end of the work, on each side, crib wings, some 30 feet long and rising to high water, were planted to break up the local currents.

These efforts were successful, though by a close shave, as when the scouring ceased, the average penetration of the piles was from 4 to 6 feet only, and this had to resist the buoyant action of the water acting against from 14 to 16 feet of vertical timber above, as well as the impact of the waves. Gradually the sand began to accumulate against the work, and in February it stood the hardest gale of the winter without flinching and gained greatly in strength, as the storm piled several feet of sand against it on the outside, throughout its length.

It was now determined to build the work without the intervention of a contractor, and active measures were immediately taken to purchase and prepare plant for energetically carrying it on in the spring. This plant comprised—six engines and pile-drivers, blacksmiths', carpenters' and caulkers' tools, chains, ropes, boats, oars, assorted iron, horses, horse

power, pump, pipe, hose, blocks and tackles, &c., &c.; in short, everything necessary for prosecuting a large work nearly 500 miles from a machine shop, foundry and base of supplies—San Francisco.

The drivers were built of choice pieces of well seasoned Oregon fir, and were well braced and bolted; the gins were ironed with ¾ inch wrought iron, and each machine had two sets of rollers, one set (4 rollers) attached to the main ground sill, the other set (4 rollers) to two heavy, strong frames, upon which the whole driver rested; this allowed two motions at right angles to each other. The rollers were made of seasoned California laurel, a very strong, tough and hard wood, and were banded and the bar holes fitted with iron thimbles. The sill which rested on the roller frames was large enough to carry the engine, a 50 gallons' water tank, and a small coal box, so that everything moved together. No nipping-off chocks were provided at the top of the gins, as it was designed in all cases to use a loftsman to snap the hammer at a certain height. The timber in general length was 24 feet, and was on the ground and had to be used, while the scour threatened to give us a depth of water greater than that for which the piles were intended, so it became necessary to avail ourselves of every inch of the piles, driving each with care, so as to get its head flush with the top level of the work without splitting or brooming; hence a low fall of the hammer in all cases.

One engine was second-hand and with horizontal boiler, the other five were new and of the latest design, having upright boilers, horizontal engines, short horizontal throttle levers, 6 inch-cylinders, 12 inch-stroke, winding drums and gypseys; everything was very compact and convenient. These engines did their work remarkably well, being exceedingly quick in their action, so much so that it was difficult to get competent drivers who would not break cogs, nor let the engines run away with them. Driving piles with a quick engine, requires the faculties and body to be constantly on the *qui vive*, eye, hands, feet and brain, and I changed drivers three times all round before I got a competent crew. It kept a head machinist (himself one of the quickest and neatest drivers I ever saw) constantly engaged watching the others, breaking them in, and keeping the engines in good repair. He put on each engine, in the exhaust pipe, an extra throttle lever, to act as a check in lowering the hammer and to catch it should the brake give way; these saved a number of mishaps, and ought to be on every hoisting engine.

The gins of the drivers were comparatively short, not more than 35 feet high, with three guards, the falls carried over 11 inch-cast iron shieves;

the inclined after braces ran to within a few feet of the top and abutted against solid block sides, and were cross barred to be used as a ladder. Each guard was provided with a snap bar and hammer chock, made fast by short lines to prevent accidents. The hammers were cast especially for the work from drawings furnished, and differed from the usual type, in having 18 inch-faces, so as to drive two 12 inch-sheet piles at once, one lapped 6 inches on the other; the lightest for driving scaffold piles, weighed 1 900 pounds, the rest 2 400 pounds, each. The whole plant was constructed in the most substantial manner, for very hard steady work, and was stronger than necessary, had operations with it been carried on in the vicinity of repair and supply shops. It fully answered its purpose, and by its superior quality conduced greatly to the speedy and proper completion of the work.

From the nature of the locality—a series of flats very shoal or partly bare at low water and exceedingly rough with surf at high tide—it was impossible to use floating drivers to advantage, so that all work had to be done from a scaffold.

The first or advance driver was built with an overreach of 10 feet, the sill consisting of two pieces 40 feet × 12 × 12 inches, framed together and hog-braced, with the engine and tank at the after end to counterbalance the driver and the hammer forward; the lateral roller frames were 40 feet long, the width of the scaffold, allowing the machine to traverse 32 feet, the distance between the two rows of scaffold piles.

To begin operations, a temporary floating driver was rigged, and a small staging built at the end of the short piece of work already completed by the contractor; on this scaffold (some 40 feet square) the advance machine was set up and began to build its own roadway. It first drove a 12 × 12 inch-pile, 16 feet on one side the axis, then traversed and drove one 16 feet the other side, then back to the axis where it raised a 40 feet × 12 × 12 inch-stick, and laid it on these two piles for a cap; a loosely fitting blunt bolt through each end into the pile head held this to its place. The same set of caps and bolts answered for the whole work. After the caps were on, the machine raised a lot of pieces 24 feet × 12 × 6 inches, and laid them longitudinally for roadway. These pieces were dogged lightly to the caps to keep them from being washed overboard by the surf at high water, which frequently broke over the scaffold. The bent thus built was stiffened by a 40 feet × 12 × 4

inch-brace, bolted edge up to the top of the sea side pile and running to the level of low water on the inside pile, one set of these braces and screw bolts being sufficient for the whole work. About 1 000 feet of scaffold or 100 bents were always in position, to give plenty of working room for the six machines, and *No.* 1 as a rule did little else than build it, it being about as much as it could do to keep out of the way of the other machines; it averaged 60 feet of completed scaffold a day, sometimes building as high as 80 feet. A yard gang with cant hooks and a pair of heavy draught horses rafted the timber at low water, each raft being taken out on the ebb tide by one man, who had a pole to guide it, and made it fast to a pile near the machine that was to use it. No more timber was taken out than could be used in one day, and any spare pieces on hand in the evening were always hoisted on to the scaffold to keep them from going adrift.

No. 1 machine was followed by *No.* 2, which, being of rather peculiar construction, merits description. It was built under my direction by Mr. M. P. Hubbard, superintendent of construction, from a design by himself, and was intended to drive both the vertical and brace piles. (See Fig. 1.) It had two sets of gins, one vertical and the other inclined at an angle of 30°; these latter being mounted on heavy trunnions at the lower end to permit adjustment to any angle. The two sets of gins had the usual after braces, the guard rails of the vertical gins running clear through and making guards for the slanting gins, thus strongly bracing and stiffening the whole affair. From the photograph * it would appear very unwieldy, but in fact it was not at all so, no larger crew nor longer time being required to manœuvre it than the other machines; like them the engine, &c., moved with the machine. The contractor, in building the first few hundred feet of the work, had used only one and that a single gin driver, hinged at the bottom to allow the gins to be thrown back to the angle necessary for the brace piles; but when this was done the guards had to be changed to make them level, thus losing at least half a day; the principal objection, however, was its general want of stiffness; it was too rickety. With the double machine the transfer of the hammer required but five minutes, as it was run down out of one set of gins and drawn up into the other set, each having its own fall.

* To be seen at the Society rooms.

This machine followed after *No.* 1, and drove a row of brace piles till it caught up with it; close upon its heels followed a squad of ship carpenters who beveled the heads of the braces (see Fig. 1), to allow the verticals to be driven against them. This was done also by *No.* 2, running back, changing its hammer, and going ahead till the verticals caught up with the braces. The two most experienced, intelligent and trustworthy foremen were in charge of these first two machines; the scaffold had to follow a curve, keep a certain general level, and have its bents accurately spaced so that no cap should come over the place for driving a brace and upright. To keep the centre of the scaffold approximately on the curve, each bent was placed with a given number of inches of departure, and afterwards the true axis of the work was laid out and marked by flexible battens tacked to the caps. Practically the work was built in chords of 100 feet length, which however were so short compared with the radius of the curve, that when finished, the eye failed to detect any departure from a true curve.

The scaffold level was checked every 300 feet by a Y level, and the true level for the top of the upper stringers was scored on every fourth scaffold pile, from which it was carried by the carpenters with straight edge and spirit level to the work.

The foreman of *No.* 2 had to look carefully to the spacing of the brace piles, and see that they did not creep or twist, as they were too stiff to be sprung into place. Their curvilinear alignment was superb, and they drove more readily and truly than any other piles in the work, their slope being uniform throughout. The scale distance on the drawings from centre to centre of verticals was 5 feet, but an inch was allowed over this, for swelling of sheet piles due to absorption of water in rafting. In driving the verticals, care was taken to have them abut squarely against the bevel on the brace heads, to insure an even bearing, and out of some 1 150 of these 12×12 inch-uprights, but 2 were noticeably twisted.

As fast as the verticals were driven, a squad of ship carpenters sawed the brace heads off square to give a close even bearing for the stringer, and bolted them to the principals, (see Fig. 1), and also fastened on the upper and lower stringers. The gains were made in the piles in all cases, and were 1 inch deep; the scribing was done from the stringers themselves, which were then hung in chains till the gains were cut, after which they were replaced and held in position with heavy iron ship-clamps, made for this work, until securely bolted; a snug, accurate fit

was thus secured. The lower stringers were put on at lowest low water, the carpenters often being out as early as 1 o'clock A.M.—the low water of the spring tides occurring in summer at night.

The stringers being on, the other four machines followed and drove sheet piling; the intervals between them were adjusted to allow each machine to close its interval in two or three days, and during spring tides, these intervals were so short as to allow closing on each other every night; this to some extent prevented a scour from taking place during the intervals.

No. 6 worked with gins faced to the rear, and as soon as it had closed its interval, fleeted back and dismantled the scaffold, the roadway, cap and brace pieces being rafted and sent forward for re-use, and the piles pulled up, by attaching the fall and starting the engine; whenever they broke off, as many of them did, it was always flush with the sand, and the pieces were used for braces and ties in the double-work, and for chocks and wedges. This waste of timber, *i.e.*, the lengths left buried in the sand, could hardly be avoided owing to the exposed position of the work, and the great length of staging required; lighter pieces would have stood the dead weight, but not the racking motion of the machines and surf.

In building the double work, (see Fig. 2), *No.* 1 drove the sea side principals 14 × 12 inches and 8 feet apart, as it went along, and but one extra row of piles for the scaffold, these latter were also 8 feet apart instead of 10 feet.

In driving the sheeting the following method was adopted. (See Fig. 1.) The 4 × 6 inches, *1* unbanded, was spiked lightly to the 6 × 12 inches, *1* banded and these two *1 1* were driven together. A slight bevel on the free side of the foot of the 6 × 12 inches, caused them to hug the main pile. Next *2-2* were driven together though not spiked to each other; each had a band, and the 18 inch-hammer face covered both and gave an equally bearing blow; these were also beveled to make them hug the *1-1*, and the tops were kept hard up by the blocks and wedges; *3-3* were next driven together like *1-1*, and *4-4* like *2-2*; then *5* was put in by itself and acted as a shutter to key up the whole panel tight and close.

One ship carpenter to two machines was kept busy making long wedges to fit cracks that would occur, and in making chocks and small wedges. The practical water tightness of the work thus secured more than repaid the cost of the carpenters. The wedges were forced into

place by lowering the hammer on to them, and were sometimes 20 feet long and only 2 inches thick at the but.

When a scaffold had been removed, a squad of carpenters followed and trimmed the work to level where necessary: they then built a light tramway of 3 feet gauge and some 2 feet above top level; the rails were 24 feet × 6 × 4 inch-timbers, fastened on cross-pieces laid every 8 feet, and resting on a pile head left for the purpose and braced by struts from the under side, at the ends, to the top of stringers. On this tramway, light truck cars were run by hand, and carried bolts, nuts, washers, tools, &c.; 2 inch-planks laid along the centre gave a walk for the men. The heavy surf frequently broke this track: its repair however took but little time and labor.

The driving was very hard, the bottom being of firm, compact sand, with only occasional soft spots; the average depth of penetration of each pile at the time it was driven, was 15 feet. The average fall of hammers was 10 feet; the piles would penetrate only so much—1 to 1½ inches—after the first few blows, no matter how high the hammer might be raised, the extra force due to a high fall being expended in breaking up the pile head; the rule was therefore a fall of not over 10 feet; in this way the average length of pile head sawed off did not exceed 8 inches, and out of some 12 000 piles, 1 000 perhaps can be found with their original heads showing the band marks and giving no sign of crack or broom, no sawing off having been needed. Careful blocking below and chocking in the gins above, to prevent lateral motion of the pile head, greatly aided in lessening the shattering tendency of the blows; quite a point, as often it required 250 blows to get a pile down 15 feet.

Early in the work, careful experiments were made on the matter of pointed and unpointed piles. It was found that under like conditions of cross-section and bottom hardness, it took fully as long—that is, as many blows, fall and weight of hammer being equal—to drive a pointed pile as it did an unpointed one, with this against the former, that unless accurately, that is symmetrically pointed, the pile would creep or twist, and if it encountered any small obstacles would mash at the point and drive still more unevenly; and the cost of pointing, if carefully done to ensure true driving, would be no small item. Unpointed piles therefore had decidedly the advantage, and, excepting the sheet-pile bevels, all in the work were subsequently driven unpointed, care being taken to see that the foot of the pile was square with the sides, to keep it from creeping or twisting.

The pile-driver crews consisted each of one foreman, one engine-driver, one fireman, one loftsman and four pile-driver hands. The fireman was necessary to collect and split the cuttings which were burnt with the coal in the engines, and to keep the water tank supplied. The ship-carpenter gang averaged twenty men : then there were, the yard and rafting gangs, two blacksmiths and helpers, machinist and helper, and a large force of laborers, loading, lightering and depositing ballast against the work ; the largest force at any one time was 160 men, including superintendent and clerk.

The first problem to be solved in making preparations for the work, was that of fresh water supply ; Wilmington, over two miles distant, being the nearest available source. The possible maximum amount needed was 8 000 gallons a day for drinking, cooking, washing and running the engines. To obtain this with certainty it was arranged with the Railroad Company at Wilmington to build a tank on the wharf of sufficient capacity to hold 10 000 gallons, and to keep it full by a pump connected with their stationary engine. This tank was built, and it was provided with a 4½ inches discharge pipe, to empty it rapidly ; at the same time, a water boat was built by us 40 feet long, 10 feet beam and 4 feet draught when submerged to the deck ; flat bottom, square sides and ends, and with a 6 inch-air space all round the inside tank to give buoyancy when full. The tank was divided by board partitions into 6 chambers, to keep the water from shifting suddenly and careening the boat; this tank held about 9 000 gallons. One man constituted the crew ; he was thoroughly familiar with the channel and set of the currents. He was expected to keep the yard tank full, running day or night as the tides dictated. With a pole to keep the craft in the channel, he would go up on the last of the flood, fill the boat from the wharf tank, come back on the first of the ebb, make fast to the yard tank, and with an old horse kept for the purpose, run the horse-power and pump till the boat was emptied.

The yard tank, founded on piles near the edge of deep water, had a bottom elevation of 20 feet above the scaffold of the work, and when full gave a head of 32 feet. The conditions to be met in designing this tank was a possible ultimate need of a delivery of 5½ gallons a minute through a 1½ inch-pipe 2 200 yards long, with several bends. As it subsequently happened, the work requiring engine power extended only 5 000 feet from the tank, and at this distance there was always an abundant supply of water, even when the head was only a little over 20 feet.

The tank was $11 \times 12 \times 12$ feet, and held about 12 000 gallons. It was built of 4 inch-planks laid on edge horizontally, and bolted through; ⅜ inch wrought iron tie-rods ran across the top and centre in two directions, and were fastened to 8×8 inch-vertical pieces outside the planking. The inside was caulked and pitched and was very tight. Near the tank a platform was laid, and a horse-power fixed to it, connecting with a lift and force pump, which with a 2½ inch-suction pipe and a 2 inch-delivery pipe, constituted the pumping plant. A 1½ inch-(inside diameter) composition pipe led from the bottom of the tank down under the sand to the initial point of the work, whence it ran along on top of the outer stringer, to which it was fastened by staples, to the middle of the scaffold, where it connected with some 500 feet of hose, the free end of which could be moved back and forth to fill the engine tanks. Another pipe, 1 inch in diameter, led from the tank to the men's quarters and kitchen. The whole arrangement for water supply proved economical, convenient, and in every way satisfactory. The water cost us, including 1.1 mills a gallon paid the Railroad Company, labor and horse expenses, and capitalizing the cost of all plant at one per cent. a month, a trifle less than 2¼ mills, coin, per gallon; this is an amount considerably less than what it cost shipping along the San Francisco water front.

Active operations were begun in the latter part of May, one machine following another as fast as room could be had on the scaffold; by the end of June, all six were hammering away, and from this on, the work was driven along almost uninterruptedly till the end of October, when the last machine was removed from the outer end of the double-work, 4 700 feet from the initial point, and the whole timber work was completed; it had taken just five full months of steady work; 12 000 piles of all kinds had been driven, and over 4 miles of stringers bolted on, consuming 2 750 000 feet *B. M.* of timber, and 137 000 pounds of iron. The best day's work was 60 feet of single work entirely finished; the average was 45 feet a day of ten hours. 17 brace or vertical piles, for *No.* 2 machine, and 26 sheet piles for the other machines, was found to be a good day's work. Two sheet piles lapped and struck together, drove better than a single one, and gave snugger work. It was found that in driving the 4 inch sheet pile as a shutter, much buckling took place, so that it had to be tenderly handled; it would have been better had the two courses of sheet piles been 5 inches each, instead of one having been 6 and other 4 inches.

No accident more serious than mashing a finger or toe, due to the sufferers' own carelessness, happened during this or any subsequent work carried on under my executive charge, a matter for congratulation considering the great amount of heavy timber handled, and the danger of drowning to men falling overboard or swept off the top of the work when going back and forth, things that frequently happened. At high tide, when rough, large masses of water would sweep over the top of the work, or striking on the side be projected 20 or 30 feet in the air, the men had to watch out for these places, and dodge by them between times; sometimes they missed it and went overboard. The moral effect on some was quite marked, and I had trouble in keeping certain good men on this account. The non-swimmers, if they desired, were always sent out in a boat, though even with the latter, it was often difficult to effect a landing at the scaffold.

It will be remembered that the piece of work built in the preceding fall had been left to stand through the winter; by spring, the currents round this had accommodated themselves to the new regimen, and the work had increased in stability from the sand piled against it by the winter gales; but as soon as the new work started across the path of the tidal currents, the engineering difficulties began. The ebb tide, striking the work broadside on, would head up, and with increased velocity run along the inside of the work, scouring a deep channel of V shape, the apex being against the piling, and find its way round the free end where it would scour out deep holes. The flood tide effected the same thing on the outside and deepened the pot-hole at the end; the lateral scouring was in itself not so serious as the hole at the head of the work, which, keeping along in advance, left behind a deep cut. I had anticipated this action, and to provide against it, at intervals of 150 feet on each side of the main work, pile wings or groins were built, each about 30 feet long and at right angles to the main work. These to a great extent alleviated the lateral scour, but did not help the scour in advance, and I erred in making them too light; they consisted of 8 × 8 inch-uprights, with one course of double stringers 6 × 6 inches, and but one thickness of 4 inch-sheeting; this left many small cracks through which the water worked, and resulted in undermining the wings themselves, all on the sea side being carried away entirely, and quite a number on the inside of the work. One of double sheeting, with heavy principals and double strung, and 40 feet long, was built, and stood well, though more exposed by far than any of the rest, and it contributed greatly in stopping the extension

of the largest of the three breaks that afterwards occurred in the main work. The condition of the work in many places appeared critical, and the whole completed line as it progressed, required constant watching, the deeper places being ballasted with boulders, but my means of obtaining these were at the time very inadequate.

Every day, soundings were taken every 5 feet on both sides the work, and these were plotted on a long roll of profile paper each evening; each foreman was required to mark in red chalk on the top of every pile the length cut off after driving, and these were noted and handed in daily, and also plotted on the profile sheet, showing the penetration of each pile. From this by a glance, I could tell every day the exact condition of the work, and the depths or contour of the bottom on each side. This paper was at all times an interesting and often a perplexing study, and was invaluable in showing me where to apply to the best advantage my limited resources for protection, such as sinking box cribs, sand bags, and riprapping with boulders and gravel. In many places, the water was 18 feet deep at *M. L. W.*, and in a few, over 20 feet. Fortunately at these points, I had taken the precaution of driving down 30 feet sheeting, of which we had a limited supply, instead of 24 feet stuff. The curve of the bottom was very irregular, rising and lowering in correspondence with the state of the tides which prevailed at the time the work was built, the deep places occurring with the spring tides. In some places, my profile showed not more than 1½ or 2 feet penetration for some of the piles. At high water, with even the very slightest swell, the top of the work in places would wave like a ribbon in the wind. Finally, on July 18th, about 6 o'clock P. M., at extreme high tide, the water being over the top of the upper stringers, but perfectly smooth, the work began to bulge up in three places, and slowly but steadily rose straight up in the air, presenting the appearance of a solid built arched truss, till the top was about 6 feet above the general level of the work: the men were coming in from work, and some passed over these places while they were rising, and one passed over after one place had reached its highest point. This all transpired during slack water, and so wonderfully strong was the work that nothing gave way—though the 12×12 inch-stringers bent like poles—till the tide began to run ebb, and then the breaking up began; the stringers were twisted off and bolts pulled out, and the work at these places went out to sea in large panels of 20 and 30 feet in length. The destruction continued through most of the ebb tide, panel after panel giving way as the currents undermined the edges of the breaks. In the

morning there were three clean gaps of 130, 80, and 150 feet respectively, (at *a*, *b*, *c*, Plate II), with from 20 to 30 feet *M. L. W.* through them, and the edges of the gaps for some 30 feet back were in a very precarious condition.

The work in advance was continued without stop, with four machines; the water boat was taken out to the scaffold and the engines supplied by buckets, till pipe connection with the tank could be made, by laying the pipe directly on the bottom, across the gaps, several hundred feet from the work. The gangs of the other two machines were used to clear away and secure the wreck, and to protect the loose ends of the work next the gaps by ballasting with stone and sinking box cribs for submerged groins; these efforts were successful, and the ends secured against undermining. A floating pile-driver was then rigged up, and two rows of piles 6 × 12 inches, 30 feet apart, were driven, making spans of 30 feet, as a foundation for light stagings across the gaps. Each pile as soon as driven was riprapped with rock, to prevent its being washed out by the tremendous current which boiled and roared through these gaps on the spring ebb tides. Across the centre of this staging the car track and foot path were carried on 40 feet × 12 × 4 inch-pieces, laid edge up and trussed, and the staging was stiffened by pieces, 40 feet × 12 × 6 inches, laid criss-cross horizontally; no vertical bracing whatever was used, so that the water encountered only the edge of the 6 × 12 inch-piles.

We next riprapped these gaps clear across along the axis, to a depth of 3 feet to enable them to maintain their status, and they were then left till November, after the main work was entirely finished. Some of the panels of the wreck were picked up 15 miles out at sea, and the rest drifted ashore. Everything was recovered, and the timber and iron were re-used. It will be noticed that these breaks took place at the points *a*, *b*, *c*, Plate II, corresponding to *K*, *L*, *O*, Plate I, where three small low water channels originally existed; the bottom here was much softer with more or less quicksand, and it was through these places that the strongest currents outside of the main channel were found. In going over them, 24 feet verticals and braces had been used, but before the sheeting was driven, the deepening became so marked that I drove 30 feet sheeting instead of 24 feet stuff. Had the verticals and braces been of the same length as the sheeting, it is possible that the work would have held at these places; but the water getting under the short pieces made holes one foot wide every 5 feet, and these rapidly deepened and extended till the sheeting lost its grip on the bottom.

Up to the time of this accident, we had had a continuous channel on each side of the work clear to the head at this 1 600 feet point, with from 12 to 18 feet water, *M. L. W.*, and this would undoubtedly have continued throughout its construction. The day before the break, a piece of the work, about 50 feet back from the head, began to bulge up, but fortunately the scaffold over this was not yet dismantled, and a machine was run back, the sheeting punched down some 8 feet, reinforce pieces driven in on top between the stringers, and box cribs sunk alongside; this secured this piece. The relief afforded to the advancing portion of the work by the breaks in rear was instantaneous and very marked. The whole strength of the current set through the gaps, and the sand began banking against the advanced portion of the work so fast in places that it had to be shoveled away to permit the lower stringers to be put on at their proper level, and from this time we had little or no trouble from scouring except along part of the double work.

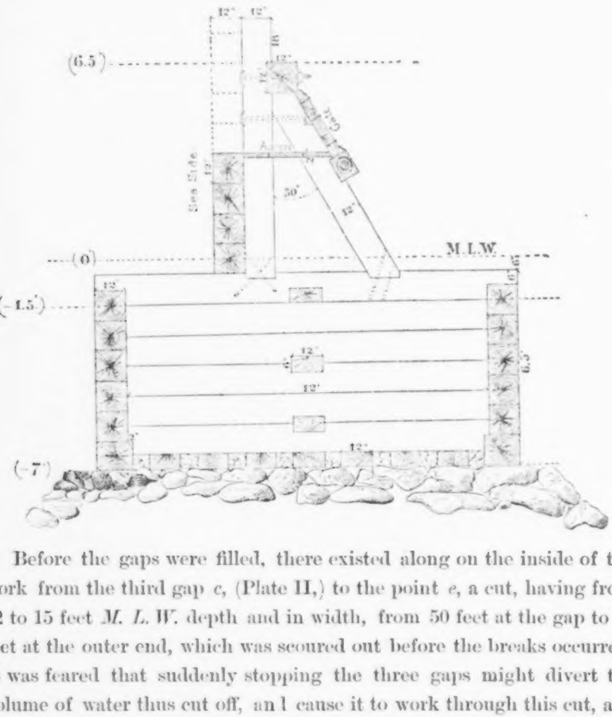
The water ways at the gaps were not regular through the channels, but only big pot holes, the greatest depth being along the axis of the work, while some 300 feet of the sand was awash at *M. L. W.*

As before stated, these gaps were left for several months till the main work should be finished, a little riprap being thrown in from time to time as it could be spared from other places. In November, the riprap was finished, with a top level 16 feet wide and 7 feet below extreme low water; upon this, as a foundation, were sunk heavy timber cribs. These in plan were 14 and 12 feet wide alternately, the former having their ends set in some 2 feet, and the latter locking into them; thus each supported the other. The ends of the end cribs in each gap had heavy 12×12 inch-pieces framed into them horizontally parallel to each other and 11 inches apart, and these projected some 3 feet and locked on to the main work. Fig. 3, (next page,) is a cross-section of the gap construction, and shows all the details. It was the strongest that could be made, and at the same time present an appearance nearly uniform with the rest of the work.

It answered its purpose fully, and never settled nor got out of alignment. The outside horizontal timbers of the superstructure at first were only carried up to half-tide; the remaining space to high water was closed by automatic wooden shutters or gates, working on horizontal trunnions fastened to the braces. These are shown on the section Fig. 3. An apron of 2 inch-plank closed the space] below the shutters between the braces and the uprights, which were spaced 5 feet apart. These

shutters opened with the flood but remained closed on the ebb tides, and allowed a large mass of water to enter, which was forced to find its way out over the bar, adding to the scour there. During the next three years as fast as the sand banked against the work on the outside, and reached these gaps and began to work over the top, they were closed up to the top level of the work and the gates removed.

Fig. 3.



Before the gaps were filled, there existed along on the inside of the work from the third gap *c*, (Plate II,) to the point *e*, a cut, having from 12 to 15 feet *M. L. W.* depth and in width, from 50 feet at the gap to 10 feet at the outer end, which was scoured out before the breaks occurred. It was feared that suddenly stopping the three gaps might divert the volume of water thus cut off, an l cause it to work through this cut, and ultimately to transfer the main channel over against the work. To guard against such an undesirable occurrence, a sheet pile wing 30 feet long, having a timber crib extension some 250 feet long with a crib T at the end (see *d*, Plate II), was built, the top being flush with that of the main work. It accomplished the intention fully and soon became the nucleus, about which gathered the sand carried along the inside of the work by the surf.

It was found that further out, over the last 1 500 feet of the single work, on the inside, the sand filled in against the piling only up to *M. L. W.;* it was kept shaved off to this level by the cutting action of the surf which rolled along against the work, whenever there there was swell coming in over the bar. At first a riprap of stone was laid down against the piling up to half tide, and collected and held the sand very well for a short time ; but the surf got such an impetus rolling along the work, that it flattened this out. I then built a lot of crib wings every 150 feet, each 50 feet long. The cribs were open enough to allow the sand to wash in, but not enough to have the stone filling washed out ; the tops were flush with the bottom of the upper stringer. These in a measure answered their purpose, the sand rapidly filling in between them and against the work from 2 feet above *M. L. W.* to half tide, which kept the worms out of the timber.

Subsequent experience fully demonstrated that these wings should have been longer, about 80 instead of 50 feet, should have been made tight, and should have had the fixed end next the work, top level with it, and been sloped off to about + 2 feet at the free end ; a fall of 6 in 80 feet. This arrangement would have caused the sand to fill in against the main work, with an easy foreslope or glacis, which would have retained its stability. As it is, the sand on the inside will fill up towards the outer end at best only to half tide ; above that the surf will keep it shaved off, and will carry the surplus sand along up to the work to *d* (Plate II), where quite an island had formed by the fall of 1875. On the land side of the double-work, four wings of heavy rock were built, and did much to hold the sand, but they should have been higher, longer and tighter. On the sea-side it was thought a low line of riprap laid against the work, would be sufficient to hold the sand piled against it by the waves, but eventually some three years after, it was found necessary to put down a series of crib wings ; the cribs had strong upright frames which extended as high as the top level of the work, but they were planked on the sides and ends only a few feet, the idea being to spike on additional planking as the sand filled flush with the top of the preceding layers, thus building the slope up gradually. This action was going on satisfactorily when I left the work in September, 1875, but I made a great mistake in not starting this arrangement three years before, as soon as the main work was finished.

We had expected that as soon as the timber work was completed, the wave action would accumulate the sand against it throughout its length,

building the slope or beach up horizontally. In this we were disappointed; this formation began at X, (Plate II,) where it filled in up to the top or +8 feet level; another place began near e, on the seaside, making a high water island, as it were, which rapidly extended bodily in both directions along the work, and also widened out in the centre to seaward. This accumulation stopped at f, but kept on in the other direction till it reached the outer edge of the third gap c, and from here started a long sickled shaped half-tide sand spit, which curved round to g; while this had been going on, the high tide island from X, had been advancing bodily along the work flush with its top, and averaging some 150 feet in width, till it reached the inner edge of the third gap, which it will be remembered had been filled in up to half-tide. This gap was now closed entirely, and this left a blind high water channel of its width at the work, which ran on a curve and joined the sea at g, where it was bare a little below half tide. It was expected that the two sand banks would continue to approach each other till they joined about the middle of the gap, and thus complete a continuous embankment clear to f; but one night during the winter of 1874-5 a heavy gale, coincident with a high spring tide, closed the beach entirely across at g, up to above high water, about +8 feet level, which level extended from f, clear round to the original 8 feet level on Rattlesnake island, and was as smooth and even as if it had been laid off with a Y level, and graded. This *coup de mer* cut off completely from the ocean, a large pool of deep water abreast of and against the third gap, in which the water rose and fell with the tide, from seepage through the sand and the riprap, under the cribs and bulkhead; but of course the filling and lowering was very slow compared with the rise and fall of the tides, so that there was often quite a difference of level between the water inside and that outside; sometimes the head of water in the pool would be as high as 3 feet.

This was an unexpected contingency, and threatened the stability of the foundation of the gap structure, which had not been designed for a dam, or to stand continuous pressure due to a head of water on one side. To secure this under these new conditions, a solid bank or causeway of cobble stones, gravel and sand was built across the gap on each side the work, clear to the top. The long wing at d, (Plate II,) was also cut away at the free end, and lowered through some 200 feet of its length, to allow some of the sand gathered here to be carried along by the surf and flood tides, and be filled into the pool and against the causeway on the land side; this action was going on favorably when I left. The clos-

ing of the beach at *g*, was very sudden, taking place in one night; thousands of fish of all kinds and sizes were left in the pool, affording for several months a rich harvest for fishermen. With a heavy surf at very high water, the waves now wash across this beach into the pool, and carry with them considerable sand, which is slowly filling it up.

The bank as it advanced from *X*, always presented on its front a bold abrupt head some 20 feet wide, and at right angles to the work. In this angle, the surf would be piled up with sufficient force on every tide to heave over a large mass of sand, which remained piled against the inside of the work, and thus the filling due to wave action advanced on both sides.

Previous to this, however, in order to keep out the worms, which attacked the timber with great eagerness and rapidity, the work had been banked up on both sides to some 2 feet above *M. L. W.*, with sand and clay brought from the opposite bluff on lighters built for this purpose.

During a four years' experience in this harbor I never found the worms (the small *Teredo Navalis*) beyond 2 feet above *M. L. W.*, nor did they work below the surface of the sand and mud, and as this filled up round a pile, it smothered them and killed them out. Below the plane of + 2 feet their action was rapid and destructive, and timber was soon honeycombed.

In Orgon fir, they showed their presence quite plainly in six months, and a pile of this wood, 12 inches in diameter, and driven with the bark on, has a reliable life of only five years--that is to stand any pressure beyond' its own weight; if sawn or hewn, its life, to resist any decided strain, will be only four years. The California redwood (*Sequoia Semperrirens*) resists the worm much better, especially with the bark on, and its effective sea-water life is from two to three times that of Oregon fir under like conditions.

Over a million feet of our timber was treated by the Robbins process, a modification of the Bethel; it was impregnated, after artificial seasoning, by steam under pressure, to the depth of one inch, with the vapor of hydro-carbon oil, or about 1¾ pounds of oil to the cubic foot of timber, and cost $10 coin per thousand feet *B. M.*

It utterly failed to protect the timber from the worms, which were not more than two months longer in attacking it, than in the untreated timber, and when once in, their action seemed to be more rapid and destructive than in the latter, in fact they relished it rather more. I have no confidence, whatever, in the creosoting of timber for its preser-

vation from worm attack, and my experience and observation at Wilmington and elsewhere, leads me to consider it an utter failure; reports of *ex parte* commissioners, special engineers, and tests by samples specially prepared, to the contrary notwithstanding. It is possible that if a pile be thoroughly saturated with the oil to the extent of 10 or 12 pounds to the cubic foot, after an ample seasoning, it may stand against the attacks of marine worms for a long time, but this to my knowledge has never been tried, and its great cost makes it impracticable except for a very small work of great importance.

In Wilmington harbor, I never encountered but one species of worm; this did not exceed ¼ inch in diameter, nor 6 inches in length; it ate in every direction, though generally with the grain, but never went below the sand or mud. In San Francisco harbor, they have this and a large worm, ½ inch in diameter and several feet long; it eats below the mud up to near high water.

Plate II shows the extension of Rattlesnake island, along the work as it was in September, 1875. The first 800 feet of the new formation is well covered with small dunes, each having a vigorous growth of young sand plants; these were started from seed taken from the older plants, and strewn broadcast on the sand. In a few years this new piece will be entirely covered with green dunes, and its stability be assured. Beyond the point *f*, (Plate II,) on both sides the work, the sand is banked against it to half tide and above, so as completely to shut out the worms, and all the trouble hereafter will be that arising from the natural decay of the timber. Exposed as it is to salt water only, and having been well seasoned before use, I think its effective life may be rated at from twelve to fifteen years, four of which have now* passed, and it is expected that before decay can make any inroad on the strength of the work, there will be a wide, firm bank of sand piled against it by wave action, like that from *X*, to *f*. (Plate II.)

The details of the cost of this work will be no guide for that of some other work at other places, as it was built under exceptional circumstances, and subject to many unusual contingencies. Plant and material had to be paid for at coin rates, and be brought great distances, with high freight tariffs; extra expense was incurred for surplus plant to provide against accidents; wages were very high and paid on a coin basis, and the obligation of rating wages on the eight hour system, helped to increase the labor cost. Most of the skilled labor and a large bulk of

* July, 1876.

the ordinary, had to be brought by steamer from San Francisco at a cost of \$10, coin, per head.

Estimating roughly from general data, the detailed items not being at my command, I should say the timber work alone—calling the 1 000 feet of double work equal to 2 000 feet of single work, or all told to 5 700 feet of single pile work—cost in round numbers \$227 000 currency, or \$39.82½ per linear foot; this includes material, labor, and interest on cost of plant.

The double work proved an unnecessary affair, as the single work was quite sufficiently strong to stand even more sea than the former was exposed to; unless, indeed, the sand does not bank up as we expect along the double work; in this case it will prove of great use, as it is 12 feet thick, filled with stone to the top, and will stand effectively far longer than the single work under like circumstances.

Before the completion of the timber work it was found we would have more means at command than we expected, and it was determined to build the rest of the line, 2 000 feet, to Deadman's island, of heavy stone riprap. The top level was fixed at half tide, to give full flow to the last of the flood, while it confined and directed over the bar the last half of the ebb. Contract was made for the delivery of stone in pieces of ¼ ton (gross, 2 240 pounds) to 4 tons each; it was quarried at Catalina island, brought over in sailing vessels, and then transferred to a heavy lighter—built for the purpose and provided with derrick and engine—which laid it in place on the line. The original regular curve was deviated from to meet a reef of hard pan which ran out from Deadman's island some 500 feet to *h*, (Plate II,) and which gave a firm bottom that would not scour out in advance as the work progressed; it was also the line of shoalest water. At the end of the double work was a pot hole 150 feet wide and 14 feet deep at *M. L. W.*; from the edge of this to *h* the average depth was 4 feet, and from *h*, to the island, it gradually shoaled to zero.

Getting the stone into this line snugly and without waste through unnecessary spreading, was an affair that required great care and constant close personal supervision. The derrick lighter was used for handling the heavier pieces only, where, on account of low water, they had to be placed at the same or higher level than the deck; for the rest, a fleet of three lighters was built, each 40 × 18 × 3 feet, of 50 tons gross and three others were hired, each of about 28 tons gross, carrying capacity.

On these lighters everything was on deck, to which they were always loaded down ; they were moored on the line, and the stone dumped overboard by hand. This line was always rough, the swell and surf coming broadsides on, and washing over everything, and slewing the lighters from their fore and aft alignment, necessitating frequent renewal of ropes and relaying of anchors ; and sometimes it was so very rough that a lighter, to avoid being swamped, would have to be anchored several feet from the line, with some play, and the chance seized to heave over the stone as the vessel swung up to the line. I was always present on board one or the other, or all in succession, of the lighters while unloading, and maintained severe discipline over the crews about keeping the lighters up to the line, so that every stone should go where it was wanted and none be wasted by being thrown too far away. It was nasty, wet, tiresome work, and I was heartily glad when through with it, as were all others concerned, no doubt.

Early in the stone construction, we found that the contractor was going entirely too slowly, and displayed no disposition to hurry affairs, so we went into open market and got stone from all possible sources : heavy beach boulders were gathered from the shore above San Pedro, and rock was quarried from Deadman's island. My first effort was to get a thin layer of stone clear across to the reef, at *h*, thus securing the bottom and preventing a scour, that would materially increase the cost. In this I was quite successful : getting the supply forces well organized, we were enabled for a while to put in six lighter loads a day, or about 250 tons. Each discharged half its load on the line over its length of 40 feet, and was then moved ahead its length, and the other half unloaded over another length of 40 feet, each lighter closing its load on that of the preceding one, so as to leave no gaps. The whole length of line to *h*—some 1 500 feet—was secured in six days, and during neap tides, and no cutting took place. By this manœuvre we saved 20 000 tons over the original estimate, which allowed for scouring and settling. The line was raised to a uniform height of 4 feet above *M. L. W.*, one foot extra being given to allow for settling. The boulders were put down first, and covered with the heavy stone : in many places these latter were afterwards knocked off, but the boulders stood the heavy surf remarkably well ; being covered with barnacles, they locked into each other, and the seagrass growing on them very rapidly, aided in holding them together.

The line had a top width of from 12 to 20 feet, with natural slopes, and in less than two years after it was built, the sand had filled in against

it on both sides, all along up to and in places above *M. L. W.* It consumed 13 000 tons gross, of stone, and was constructed in one working season. It cost about \$16, coin, per lineal foot.

It admirably answered the purpose for which it was designed; *i. e.*, to check the ebb tides, and cause them to exert their strength on a line across the bar, while it allowed free influx to the flood for filling the tidal reservoir inside the bay.

I found in one of my series of current observations that, after the ebb tide had got under full headway, there being some 3 feet of water over the top of the stone line, and the current over the bar being as high as $2\frac{1}{2}$ miles an hour, or $3\frac{2}{3}$ feet per second, the current across the stone line had a velocity not exceeding in any place $\frac{1}{2}$ mile an hour, or only one-fifth of the bar velocity; showing that the friction of the irregular topped stone line on the bottom layer of water passing over it, was so great as to materially retard the surface velocity. This was so remarkable that I several times repeated the experiment, and found always the same fact. The flood velocity here I found to be greater than that at a corresponding stage of the ebb, due no doubt, to the reinforcement of the swell which nearly always prevailed.

As soon as the main timber work was completed, it was thought that a perceptible deepening of the channel would take place, and to determine the rate and amount of such erosion, several cross-section lines were selected and careful soundings made over them, at short intervals of time, but for quite a long while no commensurable effect could be detected. The current velocities were known to be greatly increased, and the water on the ebb tides was seen to be laden with sand, which was carried seaward and not brought back, for the flood water would be comparatively blue and clear; and from the top of Deadman's island, on a strong, spring ebb, the ocean, over a fan-shaped area of several thousand feet, was observed to be very turbid and discolored; but still no marked channel developed. It was evident, however, that the bar was being shaved off uniformly over its whole area, though the daily or even weekly increment of depth was too small to be measured, except by sounding tests too delicate to be applied near the wide open mouth of an estuary. Some of the sand observed in motion also came from the seaward end of the inner deep-water channel, which began now to advance bodily seaward, though very slowly. There was danger that the increased velocity would waste itself by carrying the bar out bodily and outside the mouth, where it would be much harder to deal with.

An effort was therefore made to concentrate the currents more, so as to cut a channel. The matter was studied carefully and executed cautiously. While the main stone line from *B* to *Z*, Plate II., was being pushed forward energetically, a low training wall, 300 feet long and rising to *M. L. W.*, was put in along part of the line marked, jetty No. 1 (Plate II). The beneficial effect was immediate and marked; a general and measurable deepening occurred abreast of the jetty and for several hundred feet below (by which I mean down stream, as regards ebb tide and *vice versa*), but the greater effect was immediately above. The 6 feet curve widened and advanced down, as did also the 12 feet curve, though not to the same extent. The former soon began to reach out an arm towards the free end of the jetty, and the channel threatened to open right across this end, which was not the place for it; so the jetty was extended some 300 feet to *M. L. W.*, making it 600 feet long, and the first 300 feet was raised to half tide. As the natural tendency of this training wall was to throw or deflect the whole body of the current over against the opposite reef below, making the channel too curved, and bringing it too close to the reef, a short jetty, *No.* 2, was put down, rising only to *M. L. W.* This, with the reef, made a long training wall, running out obliquely from *A*, some 1 200 feet, of which one-half was bare at half tide, and all at *M. L. W.* This jetty was built late in March and early in April, 1874, and some six months or so after the main stone line had been finished, it was about 250 feet long.

The combined effect of the main work and the two jetties was now very marked, and on the spring ebbs the water over the bar fairly boiled like a tide-rip. At intervals of two or three weeks, trial lines were sounded over, and changes noted; and at intervals of three months, elaborate surveys were made from above jetty *No.* 1 out across the bar to deep water, on cross-section lines 100 feet apart. The soundings were carefully plotted and the curves laid in. The survey of April, 1874, right after the completion of jetty *No.* 2, shows the 6 feet curve had extended from the inside to a distance of 225 feet below the line, *L A*, which averaged 400 feet in width, with 7 and 8 feet curves close behind, with increased depths for some distance ahead; also a 6 feet blind channel along, near, and parallel to the stone line, averaging 100 feet in width with places 7 and 8 feet deep, and some 800 feet long. Another 6 feet channel, with 7 and 8 feet pools, ran from round the end of jetty *No.* 2, out seaward some 900 feet, and averaged 200 feet in width. The 6 feet curve outside the bar had also made a break at a point abreast the centre, and had worked

in, some 500 feet in length and 300 feet wide. The current forces were evidently working hard, but needed further concentration; they were making several useless channels and wasting their energy. Jetty *No.* 1 was raised to half tide throughout its length, and jetty *No.* 2, which had settled a little, was raised a few inches, to keep it well up to the plane of *M. L. W.*, and was lengthened 50 feet.

The next detailed survey, made in the fall of 1874, showed further well marked changes for the better; the inside 6 feet curve was now down some 700 feet below the line *L A*, then shoaled to 5 feet for 150 feet, when there came another 6 feet curve some 400 feet long and 200 feet average width; both these 6 feet curves having 7 and 8 feet curves over much of their length and width. From jetty *No.* 2 a straight, well-defined 6 feet channel, with a 7 feet curve well up with it, ran out towards the 6 feet spur outside—this latter having shifted from its centre position over towards the land—and the two were joined by a 5 feet channel. Another 6 feet spur from the outside curve had made in some 400 feet over in front of Deadman's island. This left a broad, flat bar right in the middle of the mouth of the estuary, with channels on each side, making a *Y* shaped affair, with the stem towards the inside.

The survey of January, 1875, developed this tendency still more; the two inner 6 feet curves had joined, and the outer end was some 1 700 feet below *L A*, had a width averaging over 200 feet, with a 7 feet curve well up with it, and carried 8 feet, some 800 feet below *L*; the 9 feet curve was 300 feet below *L*; jetty *No.* 2, 6 feet channel, was now clear through, with 7 and 8 feet over most of the course, and had its mouth trending to the southwest; there were also from the head of *No.* 2, 9 and 12 feet channels of quite fair lengths.

By September, 1875, the main 6 feet channel had worked clear through to the outside, but in two branches, one going to the right and joining into jetty *No.* 2 channel, and the other bearing to the left, and joining the 6 feet spur which had cut out in front of Deadman's island; of the two, the former had the greater depth and was the wider.

In the summer of 1874, a contract for dredging had been made, the idea being to expend the balance of the funds in opening a cut through the reef between Deadman's island and San Pedro, that the tides might cut down the bar and open as deep a channel as possible. It became necessary now to determine definitely which channel should be adopted: the west or the east, and though the former was at the time the better of the two, it was thought that it was but temporarily so, and that the one

near the island would ultimately prove the more desirable. To test this matter, current observations were made, and it was found that the general set of the strongest portion or thread of the ebbs was along the axis of the east channel. The mouth of the westward or landside channel had already shown a tendency to work off to the west, owing to its being on the lee side of the bar and to the direction of the prevailing swell and winter gales; and it was apprehended that a storm would be liable at any time to drive the bar or a spur from it, right across the mouth, thus blocking it entirely, or else shift it bodily further west, making the channel very crooked and unavailable for navigation; and, moreover, this channel had a rocky reef on its lee side, so that a vessel risked a total loss if stranded.

The other channel lay under the lee of Deadman's island, which would greatly break the force of storm waves, and quickly reached deep water, which lay to windward right outside the island, and where there was no body of sand to be driven by a gale across the mouth; again, if a vessel coming in in a storm did take the bottom by drifting to leeward, it would be on a sand bar entirely, where there was greater chance of saving life and cargo; the tendency of the tides, both flood and ebb, was to set through this channel and round the island, thus promising effective aid in keeping it open.

The objection to this channel was the greater length and depth of reef-cutting to be made, augmenting the expense, but as we were not cramped as to means, this became a minor fault; the points in its favor being so many and marked, it was adopted, and subsequent developments proved the wisdom of the selection. The survey of December, 1875, (Plate II,) shows the western or right hand 6 feet channel broken off both inside and at its mouth, leaving only a blind lead from jetty *No.* 2 part way out; while in the other channel there was $6\frac{1}{2}$ feet and most of the way 7 feet, at *M. L. W.* In fact, there is much more water along this channel than is shown on the chart, but it is due to dredging, and the aim here is to show only that naturally due to the scour of the tidal currents, concentrated and guided by the main work and the training walls.

The intention ultimately is to get from 10 to 12 feet *M. L. W.* over the bar, and more if means will permit, or as much as experience will prove the currents can keep open; this will give 16 to 18 feet of water on the high spring tides. This will afford a good second class harbor, and if properly utilized will be sufficient for a large commerce.

To preserve this depth when attained, with greater certainty, and to lessen the amount of dredging necessary, it is at present intended to continue jetty *No.* 2, out some 250 feet on its axis prolonged, and by an easy curve bring it parallel to the channel and carry it out over the bar, keeping the top at or a little below its present level.

The submerged reef gave less trouble than was anticipated, a cut 60 feet wide having been made clear through it before I left, and this will be widened to 200 feet. Charges of 25 pounds of giant powder laid on the surface and exploded at high water, were found quite sufficient to break up the material so that the dipper of the dredge could get hold of it.

I will now briefly review the results of our operations up to December, 1875.

When we began work in 1872, there was over the bar barely 1½ feet at *M. L. W.*, with no defined channel whatever. In December 1875, as the immediate result of our works, we had a straight, clearly cut, well defined 6½ feet *M. L. W.* channel, well situated, with fair prospects of continued improvement. The 6 feet curve averaged 250 feet in width, and through most of its length across the bar, 7 feet at smooth water could be carried. This shows an absolute gain of 5 feet of water on the bar. At first sight this may seem insignificant, but a moment's consideration will convince that a 5 feet increment on a bar is no small matter. According to the rule hereintofore cited, that the tonnage capacities of different harbors vary directly as the cubes of their bar depths, it will be seen that this increase of 5 feet has rendered the capacity of this harbor, for tonnage, over 81 times as great as it was originally. This has been accomplished by tidal currents alone and acting on a hard drift bar, and not by the aid of a river of constant velocity and direction and acting over a soft muddy bottom, where of course greater absolute results might be expected.

It is generally advanced, I believe, that eddies tend to produce shoaling in their immediate vicinity ; this is undoubtedly true where the eddy is of large surface diameter and slow velocity at the perimeter ; but small eddies of from 2 to 8 feet surface diameter and of high rotary velocity aid greatly, I am led by my observation to believe, in effecting scour, by their augur like or boring action on the bottom. I noticed that on strong ebb tides, the water would be full of small eddies, in each of which the sand could be seen boiling up from the bottom or apex of the eddy, and in one of these I have measured an increase of 6 to 8 inches in depth in an hour ; but the principal

ground for this belief is the fact that on the Wilmington bar, where a scour occurred, the bottom became pitted, first with small pot holes, 6 to 8 inches below the general surface, and of about 2 feet top diameter; this was invariable, and was always noted and accepted as the precursor of a general deepening. These pot holes would finally join or run into each other, and then a well-defined cut, with smooth, uniform bottom, would obtain, my very frequent and close sounding surveys enabling me to study this action intelligently. Did not the expense forbid, a publication of these successive surveys would prove an interesting and instructive study as to the gradual formation of a channel under current action, and those resident members who are interested especially in this matter, may find it worth their while to examine the different tracings and photographs left at the rooms of the Society.

Engineering authorities give a number of tables of current velocities necessary to move certain materials, as sand, mud, clay, gravel, &c. These tables are approximately alike, and the velocities given by them, especially Du Buat's, are, I think, all too small to be relied on in practice. They are generally founded on experiments in smooth troughs or small channels, with both material and water in small quantities. In practice we have to deal with both, in large masses and moving over irregular surfaces. My own observation leads me to believe that the tabular velocities in practice should be doubled, at least, in calculating definite results to be expected from increased current velocities, *i. e.*, instead of expecting fine sand to be scoured out by a current of ½ mile per hour, the engineer had better plan to raise the velocity to 1 mile an hour, and then give his stream a generous amount of time wherein to do the work.

In conclusion, I would say that the credit for the success of the work described in this paper belongs to George H. Mendell, Major, Corps Engineers, U. S. A., under whose general direction it was executed, and without whose advice or assent no step of importance was taken.*

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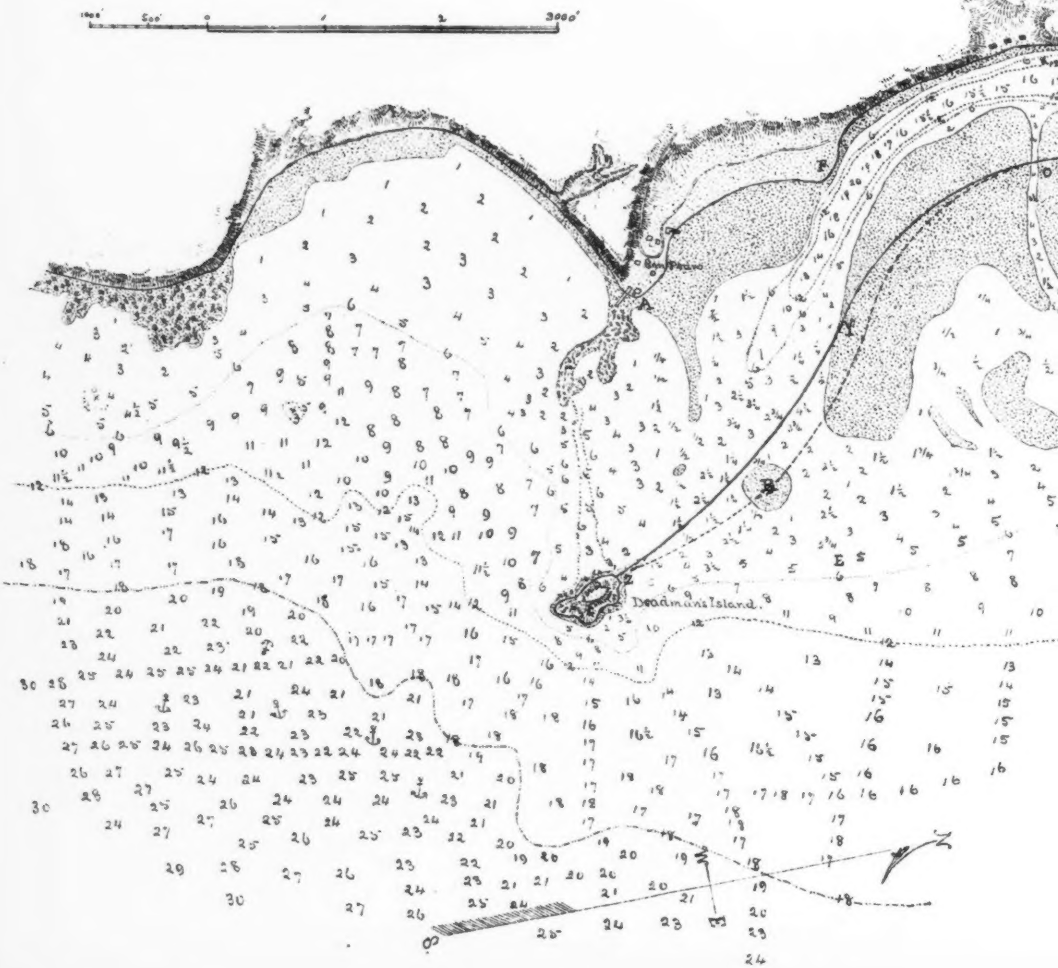
* This paper was prepared at odd moments during the past winter and spring. After it had been finished some weeks I received the printed copy of the Report as to the Practicability of the Improvement of the Entrance to Cumberland Sound, by Gen. Q. A. Gillmore, Lieut. Colonel, Corps Engineers, U. S. A., (Member of the Society), published as Senate Ex. Doc. No. 60. This report covers a good deal of matter touched on in this paper, relating to tidal action and effect of training walls, and contains a very able, comprehensive, and exhaustive theoretical discussion of the subject; to those desirous of studying this matter further I would recommend it.

WILMINGTON HARBOR, CAL.

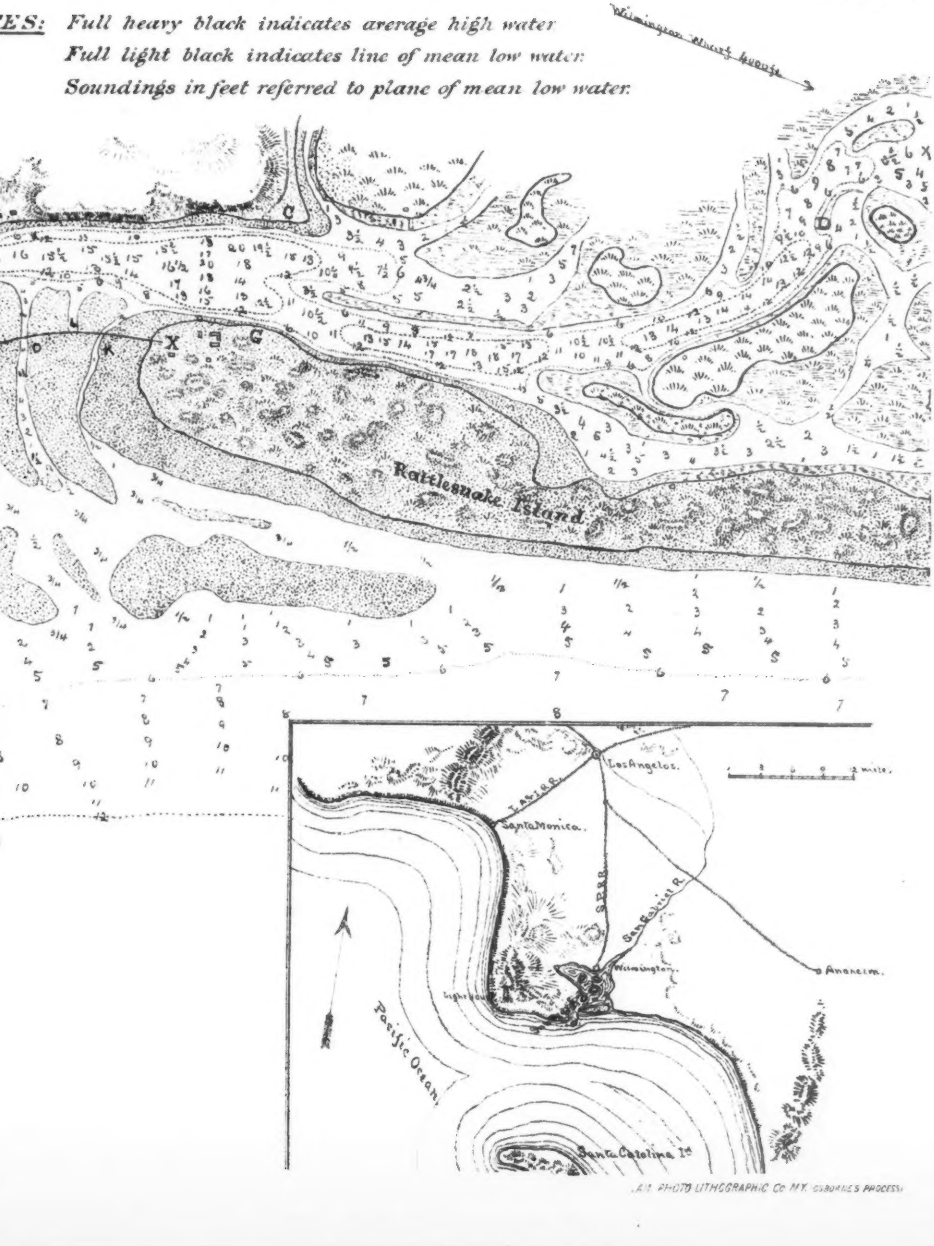
Reduced from Coast Survey Map of 1859,
and modified from preliminary survey
of May 1871.



NOTES:

Plate I.

ES: *Full heavy black indicates average high water*
Full light black indicates line of mean low water.
Soundings in feet referred to plane of mean low water.



(AM. PHOTO LITHOGRAPHIC Co. N.Y. (OSBORNE'S PROCESS)

WILMINGTON HARBOR, CAL.

Compiled from Surveys of '72-'73-'74-'75.

The 6 ft. curve across the bar is calculated as found from surveys in Dec. '75.

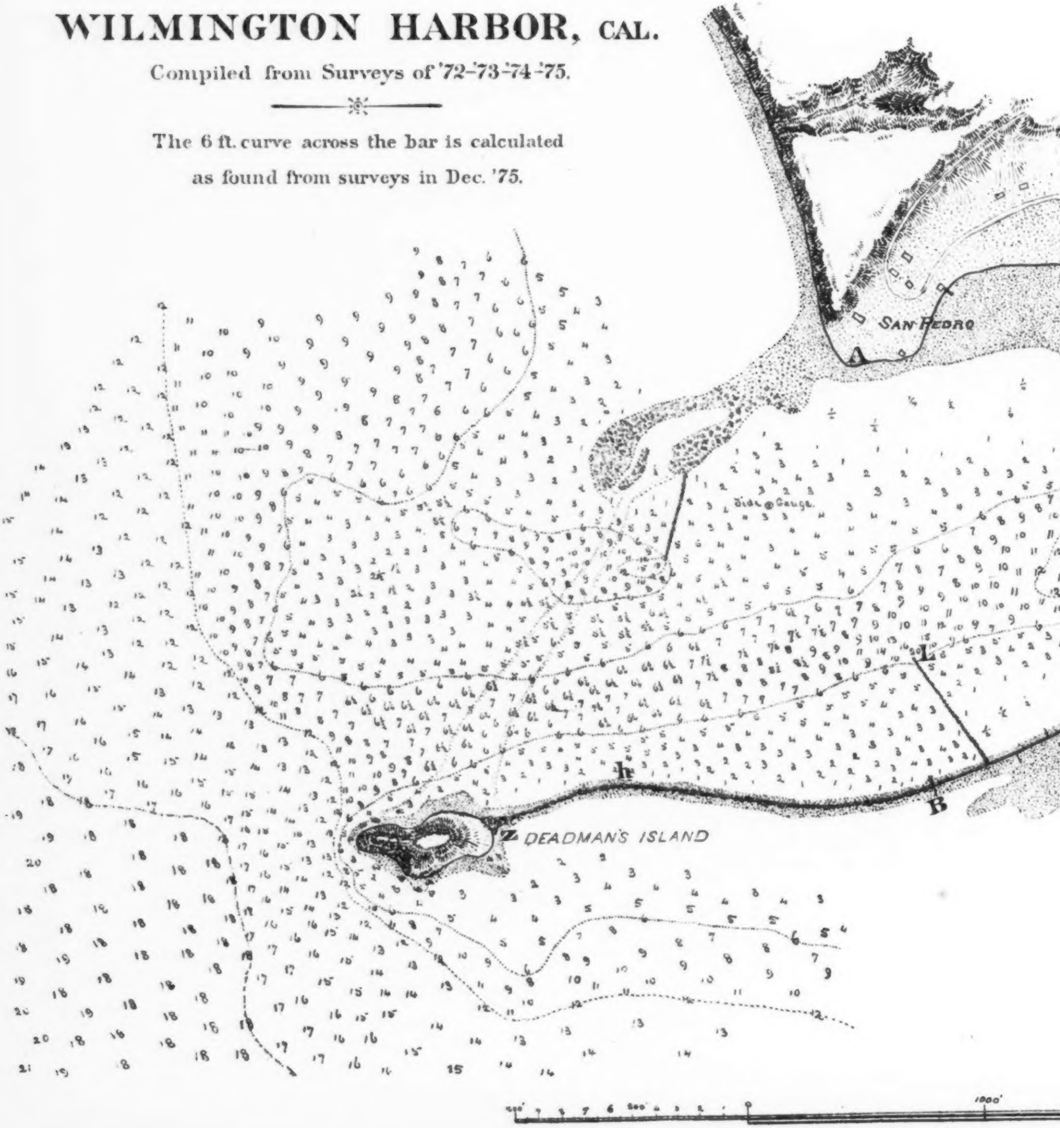
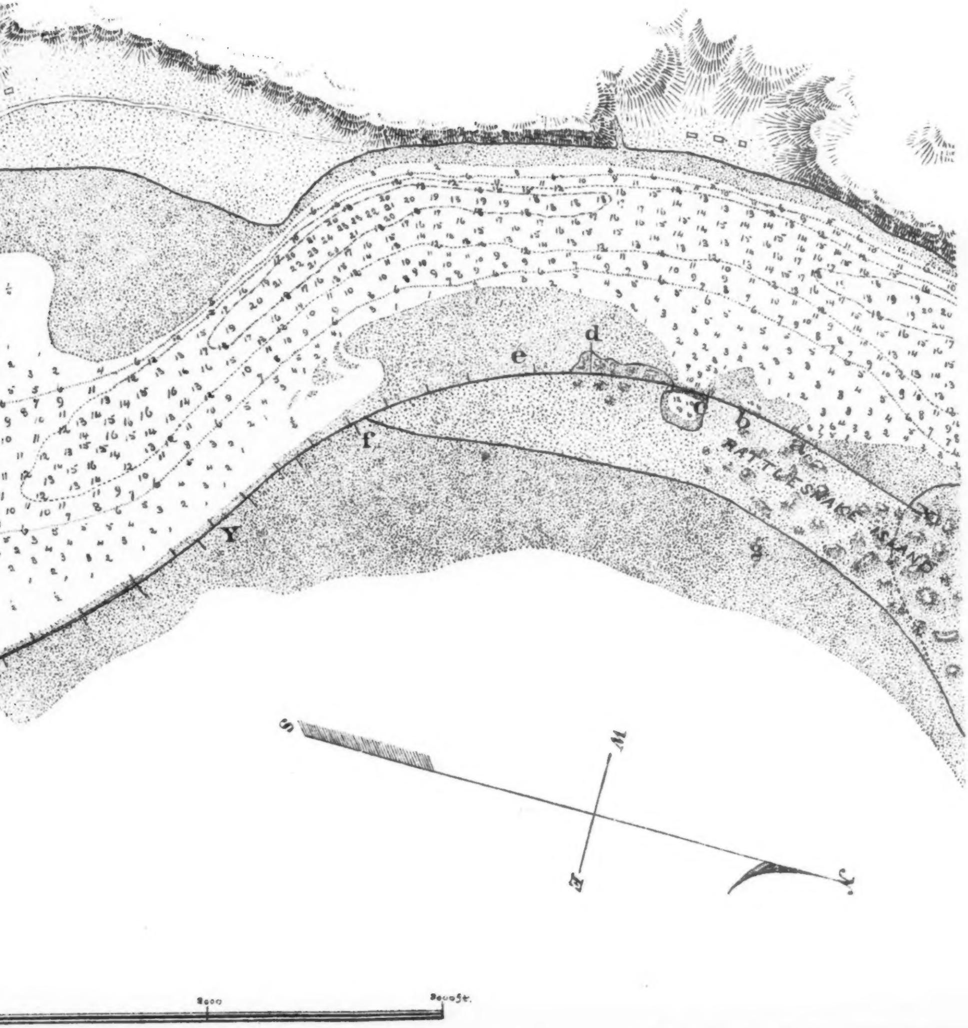


Plate II.

NOTES: *Soundings in feet— referred to plane of mean low water.*
The full black indicates average high water.



AM. PHOTO-LITHOGRAPHIC CO N.Y. (OSBORNE'S PROCESS)